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Ž. Vesel; H. Beikbaghan; B. Thierry de Crussol Des Epesse (Editors): *La science dans le monde iranien: À l'époque islamique*

La science dans le monde iranien: À l'époque islamique by Ž. Vesel; H. Beikbaghan;

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BOOK REVIEWS

■ General

Sally Gregory Kohlstedt (Editor). *History of Women in the Sciences: Readings from Isis*. 379 pp., illus., tables, index. Chicago: University of Chicago Press, 1999. \$45 (cloth); \$20 (paper).

Carolyn Merchant, in the 1982 essay that opens this volume, challenges historians of science to consider feminist approaches to their subjects because of the potential they offer for creating new syntheses in the field, redefining periodization in the history of science, and suggesting alternative interpretations and explanations. Indeed, historians of women have long understood that women's history not only illuminates the actions and achievements of women but also, and equally important, opens up the past for a reexamination of society as a whole. As Merchant states, what has looked like scientific or technological advancement from a "male point of view may appear retrograde when women's issues are included" (p. 22).

This collection of essays published in *Isis* over the past two decades proceeds to illuminate just such possibilities, spanning five centuries, two continents, and disciplines from natural philosophy to genetics. The familiar but important themes of sex segregation within the professions, single-sex education, and women's "natural" attachment to certain fields appear alongside broader and often more complex issues related to female subcultures and networks that operated within the dominant structures, the influence of domestic life on scientific development, and the legitimacy of public-private dichotomies in science. For instance, several essays note how women were frequently hired as researchers, "computers," and assistants in fields such as astronomy and plant pathology. In early modern Europe, these positions were often filled by the wife or sister of the scientist. Other examples of what Margaret Rossiter calls "territorial" sex segregation occurred in the fields of physiology in the nineteenth century and home economics in the twentieth. In fact, women's colleges provided a supportive home for women physiologists, their students, and physiology both in its earlier orientation toward personal hygiene and in its modern incarnation as a biomedical laboratory science. And herein lies progress: as Rossiter points out in her essay "'Women's Work' in Science," by the twentieth century, at least,

women's expertise in astronomy or botany was much less likely to be seen as "natural" or these fields as "inherently feminine" (p. 293). (This should be read in contrast to Lisbet Koerner's "Goethe's Botany: Lessons of a Feminine Science.")

Other essays examine less well-covered issues, including the development of female subcultures in more modern times and the question of how women in Renaissance and Enlightenment Europe used the existing system of advancement in the sciences to their advantage. In early modern Europe, the unsuccessful attempt of the astronomer Maria Winkelmann to gain appointment to the Berlin Academy ("Maria Winkelmann at the Berlin Academy," by Londa Schiebinger) and the more successful strategies of Laura Bassi ("Science as a Career in Enlightenment Italy," by Paula Findlen) illustrate the efforts of women to use existing structures to achieve formal status and recognition in their areas of expertise. In twentieth-century Great Britain ("'A Lab of One's Own': The Balfour Biological Laboratory for Women at Cambridge University," by Marsha L. Richmond) and the United States ("Physiology in American Women's Colleges: The Rise and Decline of a Female Subculture," by Toby A. Appel), women used the sex segregation of professional science to create their own successful female networks. Finally, the antebellum ideology of republicanism that promoted scientific knowledge gave girls opportunities beyond gender expectations (see Sally Gregory Kohlstedt's "Parlors, Primers, and Public Schooling: Education for Science in Nineteenth-Century America").

Perhaps most suggestive are the essays that deal with the intersection of public and private domains in science. Historians need to take note of where scientific education occurs, especially when looking at girls' education. As Kohlstedt points out, "the domestic setting for scientific discussion reflects the permeability of private and public knowledge" (p. 177), and historians ought to reconsider the broader implications of the formalization of scientific knowledge in the academy. Interestingly, this transition from domestic knowledge production to more formal venues predates antebellum America. In Elizabethan England, John Dee's practice of natural philosophy wreaked havoc on his household precisely because it occurred *in* his household. Further complicating the distinction between public

work and domestic life, Dee used the household as his laboratory. According to Deborah E. Harkness, in "Managing an Experimental Household: The Dees of Mortlake and the Practice of Natural Philosophy," "mundane domestic events were subjected to scientific observation and documentation" (p. 38)—including Dee's wife's menstrual cycles. Following the lead of historians of women, historians of the advancement of science must pay more attention to this permeable boundary.

Methodologically, many of these essays are also valuable for what they reveal about social expectations and anxieties. For instance, Kohlstedt, Deborah Jean Warner ("Science Education for Women in Antebellum America"), and M. Susan Lindee ("The American Career of Jane Marcet's *Conversations on Chemistry*, 1806–1853") draw on textbooks used to teach girls science to point to higher expectations for females in the nineteenth-century United States than Victorian propriety might suggest. Similarly, the records of professional scientific organizations, controlled by men, reveal anxieties that go beyond simple male professional jealousy aimed at women who sought admission.

That said, as with most histories of science, the essays in this volume are limited by geography and class, leaving room for further comparisons. For instance, Ann Hibner Koblitz's study of Russian women scientists in "Science, Women, and the Russian Intelligentsia" offers promising new research on the role of science in bringing about radical social changes aimed at leveling class distinctions. Another question left relatively unanswered is how women's private lives—with spouses, life partners, children—influenced their research and career choices. While many of the essays draw attention to the importance of female networks in sustaining these remarkable women in their professional lives, the unspoken assumption is that these may have been the sole source of their emotional support. If this is true, should more attention be given to the gendered ways in which male and female scientists have balanced career and personal lives?

This collection of essays will remain on my shelf. As a historian of women and of medicine, as well as a professor of women's history and women's studies, I know that the examples offered here will continue to enrich my own professional life.

ALLISON L. HEPLER

Yung Sik Kim; Francesca Bray (Editors). *Current Perspectives in the History of Science in*

East Asia. xii + 546 pp., illus., figs., tables, bibls., index. Seoul: Seoul National University Press, 1999.

Not long ago, the science historian Shigeru Nakayama argued that, as an area of study, East Asia has been relegated to the periphery of the history of science (*Constructing Knowledge in the History of Science* [Chicago, 1995]). Works such as the present volume, however, may indicate that this situation has begun to change. Made up of fifty-three essays selected from among some 120 papers originally presented at the Eighth International Conference on the History of Science in East Asia, held in Seoul in 1996, this book, as its title suggests, displays the wide range of themes and subjects of interest to scholars in the field today.

In their preface the editors notify us that there is no single overarching theme among the array of essays presented but that some heartening new trends within East Asian studies of science were evident at the conference and so are reflected here. First among these trends is an avowed reversal of the "overwhelming imbalance" of studies of science focusing on China. Well—yes and no. While the book does include several essays on Japan, Korea, and Vietnam, the essays on China outnumber all others combined four to one, depending on how one chooses to sort them. Still, the editors' claim that there now exist sufficient historical materials and analytical approaches to "challenge the monolithic notion of China as the source of the East Asian scientific tradition" (p. iii) is borne out by several of the articles included in this book.

Another trend appears to be an increasing interest in the nineteenth and the twentieth centuries, with a concomitant blurring of the once sharply drawn line dividing historiographical approaches to the modern and premodern periods. Strictly internalist studies of the premodern sciences are now being balanced by contextual analysis, more common in the study of the modern sciences, and postcolonial interpretations are providing insight into indigenous intellectual and institutional developments in the wake of the arrival of Western science in the nineteenth century. Such recent trends are reflected in the diversity of essays presented under the book's eight subsections: "Historiographical Considerations," "Intellectual Background," "Ideas and Assumptions," "Institutions," "Mathematical Sciences," "Medicine and Technology," "Medical Practitioners," and "Western Science and Scientific Transmissions."

If the book can be taken as a representative

sampling of international scholarship in this field, one might also notice some other trends not indicated by the editors. For example, scholars in Western nations appear to be much more interested in applying theory (postmodern, post-colonial, social construction of science) to their subjects than many of their colleagues in East Asia, who generally still seem to favor the traditional, linear narrative style. On occasion one finds something like an elaborate chronology, sometimes with a curious dash of stale propaganda. The essay by Park Seong-Rae, "Pride and Prejudice in the Historiography of Science in East Asia," explains that the preoccupation with nationalism that characterized East Asian history in the past continues to influence historiographical styles even today. The contrast between the approaches is readily apparent.

Nevertheless, Yung Sik Kim and Francesca Bray have presented an impressive tome featuring essays by some of the most influential scholars now active in the field. It is indeed heartening to see such a diversity of subjects and approaches well represented, from Karine Chemla's "Philosophical Reflections in Chinese Ancient Mathematical Texts: Liu Hui's Reference to the *Yijing*," to Annick Guenel's "The Vietnamese Medical System before and after the War of Independence," Morris Low's "The Architecture of Japanese Colonial Science: The Establishment of the Shanghai Science Institute," Sugiyama Shigeo's "The Implication of Language and Script for the History of Science: The Case of Japan," and the extremely interesting articles on the role of women in Chinese medicine by Zheng Jin-Sheng and Charlotte Furth. Such essays indicate that the field is in very good hands. If such a book cannot begin to propel East Asian science history toward the center of the history of science, it is difficult to imagine what will.

WALTER E. GRUNDEN

Morris Low (Editor). *Beyond Joseph Needham: Science, Technology, and Medicine in East and Southeast Asia*. (*Osiris*, 13.) 443 pp., illus., index. Chicago: University of Chicago Press, 1998. \$39 (cloth); \$25 (paper).

Joseph Needham's giant achievement was to provide a grand vision of the history of science in China as integral to the global evolution of scientific knowledge and practice. In the last twenty years his model of ecumenical universalism has given way to the critical perspectives of sociology, anthropology, and literary deconstruction, producing local, contextual, plural

"multiple histories." This book, Volume 13 of the History of Science Society's journal *Osiris*, gathers fifteen essays that show the wide range of themes and locales being addressed in the current, post-Needham intellectual climate. Some of these essays examine science in the context of culture in critical and relativistic ways incompatible with Needham's vision. Others contain suggestions that although triumphalist narratives of the spread of modern science may be dead, looking at the margins of cosmopolitan science continues to provide essential insights into patterns of global interaction.

Two essays on late imperial China suggest how the deep past can challenge older definitions of technology as value-free, instrumental means to maximize factors of production or of science as the project of arriving at increasingly accurate laws of nature. Francesca Bray, raising the banner of "technology as culture," invites us to see geomantic and neo-Confucian ways of building houses as material expressions of a social world where stability may be more valued than innovation, ritual order more important than functional efficiency. "Moral Meteorology in Late Imperial China," by Mark Elvin, suggests that the political functions of calendar making and astronomy at court both intensified technical concern with accurate prediction and dampened skeptical reflection about portents. Reading these authors, we are invited to question either the naturalness or the inevitability of the marriage of science and industrial production that has shaped our modern world.

Similarly, essays on medicine and the body interrogate the biomedical paradigms governing today's understanding of health and disease. T. J. Hinrichs's bibliographical essay on the study of Chinese medicine identifies an explosion of boundary-crossing scholarship that, by adequately historicizing the topic, reveals it as a complex plurality of practices and beliefs with a protean capacity to adapt to changing social settings and regimes of truth. Questions about the nature of medical efficacy and what defines a natural body, raised by the record of medicine in China, are also brought up by Scott Bamber's study of the symbolic rationales governing the selection of *materia medica* in Thai herbal prescriptions. Similarly, Margaret Lock's discussion of the controversies over brain death and organ transplants in contemporary Japan throws into sharp relief the way high-tech medical professionals in the West have benefited from popular belief in mind-body dualism to redefine death.

Such narratives of the social body and of tech-

nology as social practice take us outside the framework of universal science that Needham espoused, but other essays in this volume hint that it is still important to understand science as global. For example, Lewis Pyenson and Ian Hodges extend to the Southeast Asian kingdoms of Java and Siam a picture of seventeenth-century Asian monarchs as patrons of courtly star gazing, where the intersection of cosmology with statecraft fostered a circulation of astronomical and calendrical knowledge derived from Indian, Chinese, Islamic, and European sources. Computational astronomy mattered to monarchs responsible for establishing the calendar that organized dynastic time and whose subjects read portents as signs of cosmic and social order or disorder. Hodges and Pyenson are most interested in demonstrating the cosmopolitan intellectual life of these smaller Southeast Asian courts, but their material raises the broader question of why it was that here, as also in China during the seventeenth century, innovative techniques for measuring time and space did not produce cosmological knowledge capable of challenging Buddhist or Hindu worldviews.

If these essays point to the close relationship between science and the state in early modern monarchies across Asia, others trace the interrelationships of scientific knowledge and power in the colonial environment of the nineteenth and twentieth centuries. Chin Hsien-yu and Tessa Morris-Suzuki add to the now-familiar story of science in the service of empire (here with Japan as the imperialist). But other essays on twentieth-century Japan record the marginalization of Asian science produced in a country that was by no means impoverished or economically backward by global standards. James Bartholomew unravels the intricate politics of the Nobel Prize selection process that kept several worthy Japanese contenders out of the running between 1901 and the outbreak of World War II. Susan Lindee shows the political logic behind the removal of atomic bomb victim body parts to the United States and how this hindered the maturation of Japan's medical research on radiation and disease for twenty-five years after 1945.

Other authors tell of the contributions—often unrecognized—of Asian scientists to scientific innovation. Graeme Gooday and Morris Low show how in Meiji Japan educational reformers pioneered an engineering curriculum based on work-study programs and on-the-job training and how it was scientists in Japan who invented quantitative seismology, using a new automatic recording device (the seismograph) and a nationwide network of monitoring stations. Kim Dong-

won and Stuart Leslie are interested in how the Korean Advanced Institute of Science and Technology (KAIST), which lagged in the basic research vaunted by American academics, nonetheless was remarkably successful in fostering market applications while training a cadre of graduate scientists for employment in industry. In "Harvesting the Pacific: The Blue Revolution in China and the Philippines," Peter Neushul and Lawrence Badash argue that California experiments in "mariculture" (farming marine plants like kelp) failed partly through ignorance of the successful development of such farms in Chinese coastal waters and in the tidal zones of Mindanao. In Japan, the medical researchers Yamigawa Katsusaburo and Kato Gen'ichi, though internationally known, were handicapped in the competition for the Nobel Prize by the fact that their most significant experiments were conducted in Tokyo.

All of these essays show scientists in motion, traveling on a global scale. The stereotype—that such travel takes Western experts abroad as teachers and brings Asians to the cosmopolitan centers as students—here dissolves. Working in Meiji Japan there were colleagues like John Milne and his assistant Nakano Toshio (seismology), teachers and students like Henry Dyer and Takamine Jokichi or Inokuchi Ariya (engineering). The medical researcher Kitasato Shibasaburo, whose research on the tetanus toxin established the theory of "natural immunity" that became the basis for the development of immunotherapies, spent six years in Berlin with Robert Koch, founder of bacteriology, but it was his close collaboration with Emil Behring that may have cost him the Nobel Prize. The Michigan State Ph.D. Chung KunMo developed KAIST and lured dozens of other American-trained specialists back home for careers in South Korea. In China, C. K. Tseng—trained in botany in Amoy, Lingnan, and Michigan—directed the mariculture industry of the People's Republic as a state-socialist development enterprise, while the commercially funded Philippine project was partnered by Maxwell Doty, a professor from the University of Hawaii, and his former student Vincente Alvarez.

As individuals, most of these scientists were mobile globetrotters, forming relationships across national, ethnic, and linguistic boundaries. By contrast, the institutions educating and employing them were rooted, tied to place and local academic cultures—and to funding arrangements that reflected national priorities. There is a corresponding tension in the essays looked at as a whole. Each presents itself as part

of a national history: we are told about Japanese, Philippine, Indonesian, or Chinese science. The essay by Yung Sik Kim on the “isolation” of Koreanists among historians of science describes an extreme form of a general dilemma. Compelled by nationalist agendas to focus on evidence of their country’s special creativity and uniqueness, historians of Korean science have been unable to explore the history of transmission and adaptation that linked their scientific thought and practices to those of more powerful neighbors and the wider world. Kim’s eloquent plea for a richer contextualization of his subject underscores how the conventions of national history may become a straitjacket and inspires our gratitude for Joseph Needham’s more ample vision, with which the real history of science in Asia began.

CHARLOTTE FURTH

Waltraud Ernst; Bernard Harris (Editors). *Race, Science, and Medicine, 1700–1960*. (Studies in the Social History of Medicine.) x + 300 pp., illus., figs., index. London/New York: Routledge, 1999. \$90.

In his introduction to this anthology of what are essentially eleven chronologically arranged case studies “first presented at a conference on Race, Science and Medicine in Southampton, in September 1996,” Waltraud Ernst, one of the editors, observes that over the past two decades the study of race and ethnicity in the social sciences has increased to the point where it has become, in essence, an independent academic specialty. Unfortunately, however, scholars are still beset with the problem of securing consensus on the conceptualization of race. Is it biological? social? both/either/and? what? Whatever it is, it is clear—quoting D. A. Lorimer, a historian—that at its root race is “a question of power and is, therefore, whether we like it or not, profoundly political.” Continuing, Ernst goes on to say that the authors included in this volume “share an ambition to break away from and to expose some of the ‘dangerous and destructive patterns that were established when the absurdity of “race” was elevated into a central political, cultural and economic concept and endowed with a power to both determine and explain the unfolding of history”” (p. 2). This has especially been the case in the writing of colonial histories that have “had an enduring effect on representations of race, in the popular media as well as the scientific community” (p. 1). What has to be contested, he insists, are the ways scientific racism, racialized medicine, and colonial rule came together to sus-

tain themselves and each other to the detriment of native peoples and treasured notions like freedom, equality, liberty, and justice.

Still, the volume has some limitations that Ernst freely admits. For example, with the exception of the chapter by Paul Weindling, “A Virulent Strain: German Bacteriology as Scientific Racism, 1890–1920,” the essays focus “almost exclusively [on a] British view of race within Britain and its empire” (p. 12). Literary and visual materials that informed many of the debates at the conference are not included, though the book does make reference to them, nor is there consideration of topics like the eugenics movement (either in Britain or elsewhere), trans-Atlantic slavery and its consequences, Nazi genocide, or the recent practices of ethnic cleansing in Yugoslavia.

Reflecting on some of the ideas presented in this work leads me to several tentative conclusions that I have also sought to share with students in my research design courses. First, because I believe that there is a strong autobiographical element in theory construction, it makes sense to me that whenever we examine concepts, theories, paradigms, and similar constructs of consciousness, we need to find out as much as we can about their authors, especially their experiences in the world and the social and intellectual climates extant at the time of construction. For as several of these authors show, science and medicine developed under the rubric of colonialism or slavery—an everyday fact of life—in eighteenth-century U.S. society, occluding our understanding and appreciation of difference in the world. Similarly, in ascertaining information about the limitations of these devices, it is also helpful to know what assumptions were made and what items were excluded from the universe of definition at the moment of creation. Otherwise believing in or proving the existence of that which lies outside that universe is extraordinarily difficult, if not improbable.

I believe that *Race, Science, and Medicine, 1700–1960*, would be of some value for advanced courses in the history of science, technology, and medicine largely because of what it has to tell us about the sorcerer’s apprentice phenomenon—persons employed by policy makers to flesh out their desires and make them workable.

WILLIAM KING

Silvia De Renzi. *Instruments in Print: Books from the Whipple Collection*. x + 107 pp., illus., bibl. Cambridge: Whipple Museum of the History of Science, 2000.

This small volume is the catalogue of a temporary exhibition of books about scientific instruments that opened in late 2000 at the Whipple Museum of the History of Science in Cambridge. The books are from the collection of Robert S. Whipple (1871–1953), manager of the Cambridge Scientific Instrument Company, collector of both books and scientific instruments, and benefactor of the museum and its library.

Silvia De Renzi's work consists of brief essays about sixty-three books, trade cards, and catalogues that date from 1535 to 1950. Each entry includes an illustration and references to contemporary historical scholarship. The essays are loosely organized around four large themes. The first is making books—that is to say, the interactions between authors, instrument makers, engravers, printers, and booksellers that shaped what appeared in print. The author has carefully examined different editions of books and compares both texts and the use of illustrations. The second theme is reading books; here she considers such readers as those who purchase instruments for practical use, students and teachers, and those seeking entertainment. De Renzi then discusses a third theme, the use of books and special trade literature, from early trade cards to twentieth-century advertisements to sell instruments. Fourth, the volume describes several books, almost all from the twentieth century, that shaped Whipple's collecting. After these accounts of individual objects, an intriguing concluding essay considers Whipple as a book collector. It examines the role of dealers and collectors in the formation of the history of science in Britain. Large portions of this section are taken from an article De Renzi published in a volume edited by Robin Myers and Michael Harris entitled *Medicine, Mortality, and the Book Trade* (Oak Knoll, 1998), which may not have come to the attention of historians of science.

This book will be of special interest to those who want to understand how scientists and instrument makers communicate with one another, with patrons, with customers, and with the general public. More generally, it suggests the rewards that come from a close study of books not only as sources of knowledge but as physical objects, as tools of various audiences, and as the possessions of particular collectors.

PEGGY ALDRICH KIDWELL

Hans A. Baer. *Biomedicine and Alternative Healing Systems in America: Issues of Class, Race, Ethnicity, and Gender*. xii + 222 pp., tables, bibl., index. Madison: University of Wisconsin Press, 2001. \$55 (cloth); \$21.95 (paper).

Hans Baer seeks to impose a Marxist analysis on the relation of "biomedicine" to alternative medical systems in the United States during the nineteenth and twentieth centuries. "Biomedicine" is defined as a "dominative system of medicine" that emerged from the collaboration of "elite practitioners and medical researchers based at prestigious universities" and the "industrial capitalist class" at the turn of the twentieth century. The transformation of regular medicine into biomedicine assured its "political, economic, and ideological dominance over rival medical systems" (p. 31). Biomedicine was the ideal medical ideology for capitalist society in that it allowed the medical and industrial elites to focus on specific diseases and specific pathogens while ignoring or diverting attention from the social and economic causes of disease. It provided a modicum of health care that enabled the working class to keep working for the economic benefit of the elites.

The argument is familiar, but it is not well presented here. The book suffers from numerous flaws, not the least of which is the author's attempt to encompass too much in too short a text. The book reads not as a continuous narrative but as a collection of ill-fitted segments. Apart from his lengthier and more coherent treatments of osteopathy and chiropractic, Baer's examination of alternative therapies is uneven, as might be expected in a book that attempts to range from Samuel Thomson to the Agni Circle.

If its scope all but guarantees superficiality, the book similarly falls short in its historical analyses. Much of this is due to the parameters the author imposes on his argument, ignoring whatever does not support his ideological premises. For example, in attributing the triumph of biomedicine to economic and political forces, Baer ignores the stunning and successive achievements of university- and hospital-based physicians after 1880. It does not seem to have occurred to Baer that the medical profession itself may have been so enthralled with its newfound ability to determine pathogenicity and develop therapies that it lost sight for a time of the social and environmental factors that cause disease. Nor does he address the change in public opinion toward regular medicine as a result of the benefits newly to be had in hospitals and clinics and through public health initiatives. Patients were as much active consumers of the new medicine as they were the passive recipients of a medical system imposed from above.

The American Medical Association (AMA) is one of the principal villains in this story, providing Baer with another opportunity to don ideo-

logical blinders. He cannot imagine any motive other than greed and power that would move the association to lobby for stricter medical licensure or higher standards for medical school admissions and curricula. He does not even address the AMA's belated but effective campaign against the patent medicine industry, which took on new life after 1890. In like manner, his examination of the consequences of the 1910 Flexner report is unsustainable. Baer sees it solely as a power play by the biomedical hegemonists to stamp out the competition.

By the middle of the book the reader has been moved along to an examination of contemporary healing sects, from suburban New Age healing practices to folk medical systems. The author seems unconcerned with the scientific or therapeutic validity of any of these systems as long as they represent alternatives to the "American dominative medical system." Somewhere women and minorities fit into all this—either as practitioners of *santeria* or as symbols of biomedicine's proletariat (nurse midwives, physical therapists, etc.). The very existence and healthiness of alternative medicine in contemporary American society seems to confuse the argument against biomedical hegemony but, like so much else in this book, is inadequately addressed.

The book is satisfactory neither as medical history nor as medical anthropology. It ultimately serves as the vehicle for the author's vision of "an authentically holistic and pluralistic medical system" that transcends capitalism in a new "global system of eco-socialism" based on "public ownership of a predominant part of the economy," workers' democracy, an unexplained "combination of centralized and decentralized social structures," and, of course, raised ecological consciousness (p. 189). If only Erich Honeker had been a Californian.

CHRISTOPHER HOOLIHAN

M. Agnoletti; S. Anderson (Editors). *Methods and Approaches in Forest History*. (IUFRO Research Series, 3.) xiv + 281 pp., illus., figs., tables, index. Oxon, England/New York: CABI Publishing, 2000. \$90.

The International Union of Forestry Research Organizations (IUFRO) is a large international body of scientists that sponsors conferences on critical forestry-related issues. *Methods and Approaches in Forest History* is a product of a conference on history and forest resources held in Florence in 1998. The history of the world's forests and the changes occasioned by human activities are subjects that have attracted the atten-

tion of a wide variety of scholars recently. The authors of the twenty-three papers in the volume include anthropologists, botanists, ecologists, foresters, geographers, and historians. One of the objectives of the conference was to provide an overview of various methods and in the process stimulate an interdisciplinary approach to forest history.

As is the case with many conference proceedings, the quality of the papers varies considerably—from reports of work in progress to concise summaries by recognized experts in the field. The following review of some of the more prominent papers highlights many of the major themes of the conference. Mauro Agnoletti's introduction provides a good overview of the book and the development of the field of forest history over the last three hundred years. Science often operates by altering one variable while holding the other confounding variables constant. R. Ostermann and A. Reifs's and V. Robiglio's comparative analyses of limited regions in Europe differing only in their cultural history—the Vosges and the Black Forest in the first case and two alpine valleys in Italy in the second—are prime examples of the strength of the controlled case-study approach. The development of new techniques in science often provides breakthroughs or at least new perspectives in a discipline. Hansjörg Küster's paper on pollen analysis and E. Corona's discussion of tree-ring analysis are relevant in this regard as they emphasize the early complexity of trade patterns, the relationships of humans and the environment, and the extent and long-standing nature of human influences on the forest. With the exception of carbon isotope analyses and the ability to separate formerly wooded areas dominated by C_3 trees from open areas dominated by C_4 crops and grasses, most of the latest scientific and historical techniques are covered.

Forest history falls at the interface of the natural sciences and the social sciences. Its quasi-science, quasi-social science status has a number of important implications. First, the most convincing forest history studies are frequently multidisciplinary in nature, as they draw on evidence from the biological sciences (fire scars, stand structure, pollen analyses) as well as human history (forest surveys, maps, satellite images). Lars Östlund and Olle Zackrisson's and Anna-Lenna Axelsson's reviews of the forest history of boreal Sweden are notable examples. Second, the increasingly multidisciplinary nature of the field has attracted a number of adherents who do not always agree with the basic tenets of production-oriented scientific forestry.

Michael Williams's and O. Ciancio and S. Nocentini's contributions critique the idea that science is or should be entirely objective. Forestry is more than simply accumulating scientific information that allows one to grow a crop of trees as efficiently as possible. Scientific understanding must be assessed within the framework of an ethical system. Williams argues that writing history is an intellectual process that interprets events in the light of a value system. The new scientific paradigm these authors see emerging is one that emphasizes holism, an increasing recognition of the complexity of the natural world, and humility. The new field of ecosystem management is one of its by-products. They also point out that the practical result of the paradigm shift is a greater respect for indigenous land-use practices in the developing world and management for a greater variety of forest resources or ecosystem services in the developed world. The question of value systems and ultimate goals is also critical to the restoration ecologist. M. Hall points out that the ultimate goal of restoration ecologists in the United States has generally been to "re-wild" the land or restore it to its pre-settlement condition, while in Italy the goal has been to "re-garden" the land or retain beneficial human elements of the cultural landscape. He leaves open the question of whether human activities can improve on nature or whether they ultimately degrade it.

By way of summary, *Methods and Approaches in Forest History* contains several thought-provoking articles and is particularly useful as an entrée in English into the field of forest history in continental Europe. Its most serious deficit is probably the very brief, almost truncated, nature of many of the papers (five to ten pages long). I wish the authors could have provided more detail in their studies—metaphorically, could have placed leaves and flowers on their trees.

GORDON WHITNEY

Alexander Bird. *Thomas Kuhn.* (Philosophy Now.) xii + 308 pp., figs., bibl., index. Princeton, N.J.: Princeton University Press, 2000. \$39.50 (cloth); \$16.95 (paper).

Alexander Bird presents us with a carefully argued, sympathetic, but critical philosophical reading of Kuhn's model of scientific change, from *The Structure of Scientific Revolutions* to his last articles. He writes insightfully on Kuhn's concepts of normal and revolutionary science, paradigms, the idea of "seeing" different worlds (informed by additional psychological evi-

dence), incommensurability, and progress. Not surprisingly for an analytical philosopher of science, Bird is most preoccupied with Kuhn's concept of incommensurability. Through detailed examination of Kuhn's thesis in light of the development of a realist semantics by Saul Kripke and Hilary Putnam and successor doctrines that correct weaknesses in their causal theory of reference, Bird argues that most instances of incommensurability between successive scientific paradigms dissolve on closer examination. While Bird admits that occasionally scientists may adopt different exemplars that reshape their perception when the exemplars in question are heavily visual in character (e.g., skilled laboratory science), theoretical changes in science do not result in perceptual change and corresponding separate referential worlds.

Bird argues that Kuhn's apparent radicalism in adopting the incommensurability thesis resulted from his conservative appropriation of the logical empiricists' descriptivist and holist account of the meaning of scientific terms. For Carnap, the meaning of a theoretical term is given by its place in a complete framework (holism) and the descriptive properties of the term define its reference (descriptivism). Kuhn extends this picture to one where scientific terms separated by a scientific revolution can both mean different things, since they are relativized to different theoretical frameworks, and yet still continue to refer to the world, licensing Kuhn's talk of scientists operating in different worlds. Bird sees this as trying to have your cake and eat it too. Either "oxygen" and "dephlogisticated air"—not "phlogiston," as Bird suggests (p. 185)—are terms that refer to the same entity, though with different meanings; or, their meanings being different, and meaning determining reference, "phlogiston theory" does not refer at all. Either it is an incorrect theory about real substances that we continue to study or it never referred to the world at all.

At this point we run into a persisting disciplinary divide between the methods of historians and analytical philosophers of science. Bird's argument depends on sympathetically fleshing out what Kuhn could have maintained sensibly in light of the analytical distinctions and practiced intuitions of the last forty years of philosophical debate, replete with twin earths, grue/green paradoxes, and the like. The goal is to create a completely consistent account of reference and meaning against which to evaluate models of scientific change. Kuhn's position, and that of many historians and historically oriented philosophers of science, by contrast, is concerned with

elucidating the causes and consequences of scientific change in thick, historical detail, and imaginary considerations of possible differences in the reference of “water” on a twin earth are meaningless.

This becomes clear when Bird examines Kuhn’s 1990 response to realist semantics, ironically exemplifying incommensurability: where Bird sees persisting muddleheadedness and almost deliberate obfuscation in Kuhn’s effort to return to real historical examples, for Kuhn only such a method can explain science. I think many would take the point further than Kuhn, who sought to curry favor with analytical philosophers after objecting strongly to the sociological interpretations of his work. Why should we believe that there is a consistent theory of reference to be had, when “reference” is just another slippery term in human language (something the competing paradigm of structuralism/poststructuralism considers in a way completely opposed to the revolution in semantics that Bird traces)? For the historian, the question “Who discovered oxygen?” is a great examination question precisely because no definitive answer is to be had.

For investigating a philosopher like Kuhn, who insisted on the continuing importance of incommensurability despite his many later qualifications and reservations on other points, Bird’s method is especially curious. At the end of a detailed and nuanced examination showing that what Kuhn could have meant—when interpreted by antidescriptivist theories—cannot be supported, Bird admits that a “minimal realism” is, of course, assumed throughout the course of his examination, “that the entities, kinds and properties that are available for reference are standing features of the world” (p. 191). How is it reasonable to assume this as the basis for examining a theorist who argued for a many-worlds realism and denied progress except as increased puzzling? For Bird, even if minimal realism were denied, his semantics would survive unscathed since that would only show that even our best theories failed to refer (p. 206). Historians may develop a better understanding of issues of reference and modeling scientific change by reading this book, and surely historians need to engage these issues more clearly in their own work. But few (even among those of us who accept a minimal realism and are dissatisfied with Kuhn) will be enthusiastic about adopting a position so unabashedly platonic and unfalsifiable.

WILLIAM T. LYNCH

Ullica Segerstråle. *Defenders of the Truth: The Battle for Science in the Sociobiology Debate*

and *Beyond*. x + 493 pp., bibl., index. Oxford: Oxford University Press, 2000. \$35.

Of the numerous books in recent years devoted to the science wars, most take a characteristic stance on the problem of scientific realism versus antirealism and associated claims about the plausibility of science’s claims to objectivity versus epistemological relativism in science. Contributors to the science wars usually defend a position that is supposed to cut across the special sciences, thereby depicting science in generalizable terms at the expense of precision about any one science. Ullica Segerstråle’s *Defenders of the Truth* is a welcome departure from the norm, since it addresses many of the dominant themes of the science wars but concentrates solely on the sociobiology controversy.

The book is divided into three main sections, the first of which provides the historical overview of the main themes, figures, and national and cultural agendas. The nine chapters in this section ably describe the science underlying sociobiology and explore how scientific work produced at some remove from sociobiology receives different treatment once associated with it. The predominant theme is that science, regardless of whether it was originally attached to a larger, nonscientific agenda, is cast as a deliberate social program once it is judged to have social implications. This observation is equally true of cases of science advocacy and criticism. The second section of the book departs from the historical stance of the first to reexamine earlier themes from a sociology of science perspective. Its six chapters provide a detailed analysis of the interplay between scientific and nonscientific values as they are mediated and represented by individual and institutional actors. What emerges from this discussion is a view of sociobiology in which the social positioning of scientific issues is critical to the success different constituencies have in delivering their message. Science is as much about the dynamics of power as it is about truth. As for truth, it dominates the final section. Here Segerstråle looks back over twenty-five years of sociobiology and evaluates what has been gained through the debate. In Segerstråle’s view, sociobiology has been an underlying factor in the production of some very good science, work that has genuinely advanced biology. Yet the essential point to make is one not about scientific realism, but about how science figures in the historical advance of our conceptions of what constitutes human nature. The book ends with chapters in which human nature, Enlightenment rationality, and the existence of

moral truth are reconsidered in light of the sociobiology controversy. The central contention lurking among these broad themes is that the insights and challenges presented by sociobiology, if couched in terms of simple dualisms like genetic determinism or free will, obstruct a deeper understanding of the relationship between science and ethics. The nuanced view afforded by a detailed historical and sociological analysis of sociobiology casts them as sometimes friends and sometimes foes. One of the central objectives of *Defenders of the Truth* is to judge retrospectively which is which.

Segerstråle's research on the sociology of sociobiology dates back to her dissertation, completed in the early 1980s. *Defenders of the Truth* is partly a sociological analysis, but it also includes a history of sociobiology, an analysis of the main tenets of sociobiology from a philosophy of science perspective, and a foray into evolutionary ethics. Lengthy discussions and textual analyses lay down most but not all of the evidentiary bedrock of the book. Segerstråle's long-term involvement in the debate allows her to adopt what is perhaps best described as an internal perspective on it. Through this unique connection, Segerstråle's many interviews conducted over the years add another, often very intriguing, dimension to the narrative. At the best of times, the textual analysis, interviews, and argument gel into a remarkably rich interdisciplinary analysis. Less frequently, the thread of the argument is harder to follow and individual sections of the book lose their direction. Those already aware of the central issues in the sociobiology debate will likely just skip over the book's lacunae to seize on the more cogent parts of the narrative. For those coming to the sociobiology controversy for the first time, however, the rewards of this substantial book will come only with some patience. Members of both audiences will likely experience the mild frustration with *Defenders of the Truth* that I did, frustration attributable to the fact that the wealth of information and commentary it contains is less solidly grounded in a conceptual framework than one would hope. One finishes the book, aware that one has probably assented to most of its premises while reading along, without ever being quite convinced by the choice of the research methodology or compelled by the main argument.

DAVID CASTLE

John Heilbron. *Geometry Civilized: History, Culture, and Technique.* viii + 309 pp., frontis.,

illus., figs., tables, bibl., index. New York: Oxford University Press, 1998. \$65.

This book is apparently intended to be a geometry textbook for high school teachers. It contains the usual material of elementary plane (no solid) geometry, largely structured after Euclid, plus a few items about the globe and some trigonometry. Its *differentia specifica* from other texts is a great wealth of historical material and historical illustrations related to geometric theory. The theorems of geometry quite often appear as generalizations of relationships illustrated in special practical problems. Under the heading "Ad Lectorem" (pp. 43–48) John Heilbron presents his general intentions. He wants us to share his delight in finding nice, tidy proofs and his pleasure in integrating the verbal and the pictorial, not only in geometric diagrams but also in the numerous reproductions from old books and architectural photographs. Solutions are included for about 150 problems.



Aristippus, shipwrecked on Rhodes, is encouraged by geometrical drawings in the sand as a sign of civilized humanity. Reprinted from a 1703 reprint of Euclid, Opera omnia (from the frontispiece of Heilbron, Geometry Civilized).

How well does Heilbron succeed in conveying his ideas? I think he has overdone his good intentions. His text is overburdened with too many details, whether from geometry proper or from items like Vedic altars, Columbus's calculation of the distance from Spain to India, Descartes's theory of the rainbow, Native American pottery, or the geometric niceties taken from the *Ladies Diary* of the eighteenth and nineteenth centuries. The few most important theorems of elementary geometry—about the sum of the angles of a triangle, the invariance of an angle subtended by an arc in a circle, and proportional segments—are drowned in a sea of other details. Only the Pythagorean theorem gets its due.

At the risk of appearing pedantic, I must note a sampling of the more specific problems. After the list of Euclid's postulates on page 73, Heilbron uses the parallel postulate in order to prove the existence of parallels (*Elements* 1.28). In contrast, Euclid himself uses the postulate in *Elements* 1.29 to prove the uniqueness of parallels. The caption of Plate 2 (after p. 152) is grossly misleading. Actually, Oliver Byrne writes out the proofs and uses colors only for a more suggestive notation. On page 143 we read that Pythagoras lived "a century or so" before Euclid; on page 54 the author fails to note that when Euclid says (e.g., *Elements* 1.4) "triangles are equal" he means that they have equal areas. Several lists are provided under the heading "Words about Angles." In the first, on page 52, the word "vertical" is defined as "pointing away from the earth's centre"; on page 58 it denotes "an angle formed by the intersection of two straight lines"; and in the text it is used in the usual way concerning a *pair* of angles. In another list of "Words about Angles," on page 97, we are told what it means to graduate (divide an angle into equal parts), along with definitions of a limb, an alidade, a degree, a grade, and a gradette (not a girl who graduated). The caption to Figure 3.2.28, on page 105, tells us that the values of the trigonometric functions have not changed in the last three hundred years. On page 241 the author states that "pi cannot be expressed as a number, even an irrational one."

On page 224 he repeats the well-worn tale about the importance of the golden section in the arts created by the German art historian Adolf Zeising in his book *Aesthetik* (Leipzig, 1855). A short look into a book about Greek architecture (e.g., G. Gruben, *Die Tempel der Griechen* [Darmstadt, 1984]) would have told him that the Parthenon is throughout proportioned after 9:4 (rather than as the fancy drawing in Figure 5.3.9, on p. 224, would have it).

Quite obviously, the Gothic windows in Figure 5.3.44 (pp. 238–239) are not constructed according to the plan in Figure 5.3.43, as stated in the caption of the latter figure. On page 287 it is stated that it would have been easier for the Gothic masons to use Euclid's constructions than just to cut and try—an assertion that only somebody who has never done practical work can make.

Who can profitably use this book? A teacher looking for material on the cultural background of geometry will find many historical examples and pictures to choose from (albeit carefully). She or he might also want to use some of the many exercises that are provided, along with solutions. Others, however, may not see the forest for the trees.

BENNO ARTMANN

Michael David-Fox; György Péteri (Editors). *Academia in Upheaval: Origins, Transfers, and Transformations of the Communist Academic Regime in Russia and East Central Europe*. xiv + 334 pp., illus., index. Westport, Conn./London: Bergin & Garvey, 2000.

Predating (and prefiguring) the science wars, claims were advanced by both supporters and critics of the USSR that the Soviet system was producing a distinctively novel culture of science and higher learning. Soviet peculiarities in those domains certainly were the consequence of the ambitious intervention and "planning" on the part of the Bolshevik party-state, but also of the prerevolutionary heritage and of the intentional or inadvertent responses of scientists and institutions of higher learning to the new conditions.

Although the full program of the party-state—the planned creation of knowledge and the ideological harmonization of science with the regime's political and economic goals—was never realized, the editors remind us, in an excellent introduction, that Communist regimes did succeed in cultivating "an unprecedented, etatist interest in science" (p. 4) that "bolshhevized" and vastly expanded the network of Academy of Sciences institutes, institutes of higher learning, and research institutes directly subordinated to economic and military agencies. However, they hasten to add, in its pursuit of status as a hegemonic "empire of knowledge," to use the late Alex Vucinich's term, "the academy needed the new state as much as the new state needed it" (p. 9). This theme of scientists' acceptance of, if not actual collusion with and eventual dependence on, an overblown, sovietized science sector serves as a rough organizing principle for the

volume, in which the contributions of Vera Tolz, Michael David-Fox, and Paul Josephson trace the historical evolution of that relationship within the USSR.

Chapters by John Connelly, Nikolai Krementsov, and György Péteri detail how the Soviet model was exported to and assimilated in Eastern Europe. Interestingly, the story is quite similar to the Soviet one. Scientists hid behind sanctioned rhetoric and rituals (“protective coloration”) to procure resources and win points for ideological conformity even while pursuing their independent research agendas *sotto voce*. Local university traditions and norms divergent from Soviet ones were maintained in some cases, and in all cases the Soviet model was at least tweaked by the influence of local conditions. And, as shown in Péteri’s study of Albert Szent-György, the lure of a controlling position within an “empire of knowledge” led Eastern European scientists to collaborate with their Communist regimes in ways similar to the earlier Soviet scientists’ experience.

I found the most intriguing section of this anthology to be the final one, in which Stephen Fortescue, Loren Graham, and Péteri examine how efficient the Soviet-style institutionalization of science has been and whether such a system is likely to survive over the long term. Among the notable deficiencies they identify are an enormous “ballast” of minimally productive scientists on institute payrolls, a disconnect between research and application, excessive centralization, little contact between academy scientists and students, a bias rewarding seniority and conformity more than creativity, an absence of merit-based funding procedures or accountability, and a tendency toward “overstretch,” expansion of the science sector beyond the means of the economy to support it. The authors’ exceptionally well argued conclusions are not encouraging for the academy sinecures, but, as Fortescue predicts, the demise of the Soviet-style academy is “likely to be achieved through attrition rather than decisive bureaucratic defeat” (p. 249). Taken together, these essays provide a sophisticated, engrossing, edifying, and expansive view of the social and institutional history of “Soviet-style science” and should find their way into science studies syllabi.

DOUGLAS R. WEINER

Allen Thiher. *Revels in Madness: Insanity in Medicine and Literature*. (Corporealities: Discourses of Disability.) [vi] + 354 pp., index. Ann Arbor: University of Michigan Press, 2000. \$36.

In the past twenty years, literature and medicine studies has become a recognized and respected subfield of inquiry, which now boasts a journal and on-line bibliography as well as a sizable periodical and monographic body of scholarship. Arguably, the affinities between creative literature and psychiatry in particular are closer and more detailed than those between literature and any other branch of medicine: psychological theory and literary art both probe beneath the surface reality of reason in order to uncover deeper irrational or nonrational levels of human experience and cognition. Both cultural fields center the inner Self, and both study the subjectivity of individual consciousness and its relations to the external world. Both psychiatrists and literary savants explore the “darker” realms of the human mind, including a range of psychopathological states.

Most recent scholarship about the literary and the psychological has taken the form of specialized studies of particular historical periods—ancient Greece, Renaissance England, and the age of European modernism are three favorite subjects—or of individual poets, novelists, and playwrights. Doubtless because a synthetic study would require so much learning in a single author, much of the extant work consists of anthologies of essays. Allen Thiher’s *Revels in Madness: Insanity in Medicine and Literature* is therefore a welcome enrichment of the field. The scope of this book is daunting, ranging from madness in the ancient Greco-Roman world, to Christianized concepts of medieval folly, through the writings of early modern authors such as Shakespeare, Cervantes, and Descartes, and on to German Romantic philosophy, fin-de-siècle French poetry, and Freud. I particularly applaud the author’s inclusion of discussions of a series of twentieth-century imaginative writers, such as Antonin Artaud, Marguerite Duras, and Sylvia Plath.

Thiher writes with knowledge and authority about the full scope of his material. He devotes time equally to analyses of individual texts dealing with insanity, to the medical-historical environment, and to the broad cultural setting. He is highly skilled at ferreting out ideas, images, and languages of insanity embedded in past writings. He appears to be intellectually comfortable discussing poems, novels, plays, short stories, philosophy, and religion as well as medicine. His book includes many sensitive and discerning analyses. (He seems especially good on French-language figures like Diderot, Rousseau, and Baudelaire.) Thiher also shows a fine awareness of the historicity of the interrelated notions of

sanity and madness. In the face of his close and scholarly microanalyses, he never loses sight of the broad humanistic nature of the literary engagement with psychological suffering. Lucid and readable throughout, his style is refreshingly free from literary-critical jargon.

Readers in the history of science and medicine may also have a few regrets about Thiher's book. First, it reveals several glaring historiographical gaps (e.g., no citation of the publications of Otto Marx, George Mora, Henri Ellenberger, or Werner and Annemarie Leibbrand in the chapter on German Romantic medicine). Second, reflecting a preoccupation of literary criticism today, Thiher adopts a thoroughly linguistic model of reality: madness is essentially a textual trope characterized by "a fall from language," a characterization that a good many clinicians and historians—to say nothing of psychiatric sufferers—may find sterile and intellectualistic. Third, Thiher's book consists of a chronological presentation of a long sequence of writings in which each work is given its requisite paragraphs. With all linear intellectual-historical presentations, however, the danger looms of digressing into a kind of catalogue. For this reason, the most brilliant explorations of the shared terrains of literature and medicine in the past decade have in my judgment been works that concentrated in great descriptive and analytical depth on a single chronological period, works such as Louis A. Sass's *Madness and Modernism: Insanity in the Light of Modern Art, Literature, and Thought* (Basic, 1992) and Juan Rigoli's *Lire le délire: Aliénisme et littérature en France au XIXe siècle* (Fayard, 2001).

Nonetheless, *Revels in Madness* is an admirable and highly successful conspectus and the first single-authored synthetic study of its kind in a generation.

MARK S. MICALE

Peter Pesic. *Seeing Double: Shared Identities in Physics, Philosophy, and Literature*. 184 pp., illus., notes, index. Cambridge, Mass.: MIT Press, 2002. \$24.95 (cloth).

Peter Pesic has written a very entertaining short book that interweaves the historical development of philosophical, literary, and scientific notions of individuality, identity, and indistinguishability. From the perspective of a historian of science, the book seamlessly combines an accurate and well-referenced history of atomic theories of matter with literary and philosophical accounts of identity. This makes the history all the more entertaining and informative, not to mention

philosophically well grounded. From the perspective of a philosopher of science, while the discussion is kept at a popular level (as Pesic himself warns), the author provides excellent references for readers wishing to delve deeper into the contemporary philosophical debate.

Pesic begins his narrative by identifying two basic notions of individuality in Homer: individuality as an exchangeable commodity (individuals ultimately possess no "primitive thisness" by means of which they may, in principle, be distinguished) and individuality as a sacrament (individuals ultimately can be distinguished by means of a "primitive thisness" knowable only to the gods). These notions are then traced through the philosophical literature in debates between Greek atomists and Aristotelians and the development of atomic and corpuscular theories of matter from the Stoics to Descartes and Newton. Notions of identity and individuality are discussed in the philosophical contexts of Locke and Leibniz and in the literary contexts of the case of Martin Guerre (a sixteenth-century case of mistaken identity) and the works of Dostoyevsky and Conrad. Pesic then discusses the further development of atomic theories of matter, culminating in the distinguishability he finds inherent in the ontologies of Newtonian particle dynamics and statistical mechanics. The next stage of the story involves the development of classical field theories from Newton to Maxwell. Here Pesic observes that wave identity is different from classical particle identity as manifest in the phenomena of interference, a point that becomes crucial to his subsequent discussion of the development of quantum mechanics. Central to this discussion is his claim that the simplest explanation of quantum interference phenomena is in terms of an ontology of nonindividuals, objects possessing what he refers to as "identity." The book ends with a brief discussion of the development of quantum field theory and the role "identity" plays in it.

From a philosophical perspective, the central claim of the book is that positing identity for quantum particles is simpler than positing an in-principle unobservable classical "primitive thisness." One can argue with this claim. One might maintain that quantum particles are individuals (each possessing a "primitive thisness" that metaphysically distinguishes one from another) and that they do not in all circumstances possess determinate properties (in particular, determinate positions). One might in addition argue that property indeterminateness can ultimately be associated with the structure of the phase space of a quantum system in comparison with that of a

classical system. Technically, the latter forms a Boolean algebra, whereas the former forms a non-Boolean lattice. In both cases, the notion of an individual can be interpreted as remaining the same. Hence, in one respect, such a “property indeterminateness” thesis is more simple than Pesic’s “identity” thesis in that it remains closer to classical notions of individuality.

Notions of simplicity aside, Pesic’s discussion is penetrating and provocative. (Elsewhere, he has demonstrated that the Hilbert space formalism of nonrelativistic quantum mechanics can be derived from a set of postulates that can charitably be interpreted as stipulating “identity” for quantum particles.) The topics of individuality and indistinguishability are at the heart of much debate in the philosophy of physics. (Indeed, Pesic’s discussion could be extended to the field of spacetime physics, where the ontological status of spacetime points plays a central role in debates stemming from Newton and Leibniz and extending to current theories of quantum gravity.) Pesic has provided a well-rounded historical introduction to the subject.

JONATHAN BAIN

Jelle Z. de Boer; Donald T. Sanders. *Volcanoes in Human History: The Far-Reaching Effects of Major Eruptions*. 320 pp., tables, illus., maps. Princeton, N.J.: Princeton University Press, 2001. \$29.92, £19.95 (cloth).

Does the world need another book about volcanic eruptions and the havoc they wreak? The answer, for this book, is an emphatic “yes,” especially for the general reader. At first glance it might seem to cover much of the same ground as Alwyn Scarth’s *Vulcan’s Fury: Man against the Volcano* (Yale, 1999). Indeed—and not surprisingly—both books discuss some of the same eruptive episodes, those of Vesuvius in A.D. 79, Iceland (various years), Krakatua in 1883, Mont Pelée in 1902, and Mount St. Helens in 1980. However, *Volcanoes in Human History* presents four episodes not discussed in the earlier book, and there is in general more on the geological background and on the biological, climatological, and ecological consequences of the eruptions. While there are naturally some shared references, this book tends to have more secondary rather than primary references and relies more on sources written in English.

The strengths appear quickly in a first chapter that introduces terms for volcanic products and provides an introduction to plate tectonics, its relation to volcanism, and the settings where volcanoes occur. Each chapter includes a clear di-

agram of the tectonic setting and a detailed map of the volcano in question, showing the volcanic center and the location of flows and slides. Discussion of the tectonic settings is detailed and enables the reader to gain a feel for the dynamics of the region. There is more technical information in this book than in Scarth’s volume, including geochemical and geophysical data where appropriate.

As examples of more and less familiar eruptions, Jelle de Boer and Donald Sanders chose Krakatua (1883) and Tambora (1815). Both were in Indonesia (where Boer grew up) and influenced climate conditions around the world. The eruption of Tambora, although more violent and emitting a much larger volume of material, occurred at a time when global communications were far less developed than when Krakatua burst forth and thus has remained less known. The authors provide a capsule history of the islands of Indonesia and of their governance and economy at the times of the eruptions. For both eruptions the immediate and long-term geological, sociological, biological, and economic effects are traced. There was tremendous loss of life from the eruptions and associated tsunamis. There were worldwide effects as well: climate and rainfall disruptions that led to failed harvests and the epidemic spread of cholera and other diseases in weakened populations. Political bodies were destabilized by famine. Similar results are discussed for the other eruptions.

The authors have sometimes included folklore, legends, poetry, and prose inspired by the volcanic paroxysms. There is a glossary of terms. The method of handling references will be cumbersome to those accustomed to science style, although the intent is probably to be reader-friendly and not interrupt the text more than necessary. At the end of the book, for each chapter, there are sections for the relatively few endnotes, a list of references cited, and a list of related readings. While this system works within each chapter, it is tedious to discover whether a particular source is cited—a situation exacerbated by the noninclusion of authors in the index, which is reserved for those mentioned in the body of the text. Nonetheless, besides being interesting to read, *Volcanoes in Human History* clearly demonstrates that volcanism, and geology as a whole, should not be of concern only to geologists and that history is important.

SALLY NEWCOMB

Alan W. Hirshfeld. *Parallax: The Race to Measure the Cosmos*. xviii + 314 pp., illus., figs.,

bibl., index. New York: W. H. Freeman, 2001. \$23.95.

Alan Hirshfeld, an astronomer at the University of Massachusetts at Dartmouth and the Harvard College Observatory, offers an appealing presentation of the history of parallax. He defines parallax as “the apparent shift in an object’s position when viewed alternately from different vantage points” (p. xi). The significance: finding a stellar parallax is central to measuring the universe. Hirshfeld divides his account into three sections: the first lays out efforts to prove the possibility of stellar parallax, the second early attempts to detect it, and the third detailed accounts of those who succeeded.

Many readers will, as I did, find *Parallax* an entertaining, informative read. Written with flair, supplemented with simple, helpful illustrations, and liberally sprinkled with personal anecdotes and quotations from sources ranging from Pliny to Annie Dillard, Hirshfeld’s well-researched account presents tales captivating in their own right; assembled, they form an engaging narrative illuminating an important topic. To avoid disrupting the text’s flow, Hirshfeld provides endnotes marked by phrases and their page numbers rather than by superscripts. It works well, notwithstanding some vague references for the interesting quotations that head each chapter.

I have two concerns about Hirshfeld’s presentation, one general, one specific. The former involves its underlying historiographic tone. We read that ancient sky observers’ “powers of observation were severely limited; they had their eyes and they had their minds, the latter clouded by preconceptions about how the universe *should* be” (p. 3). Though Hirshfeld later backtracks—“we shouldn’t judge ancient proponents [of geocentrism] too harshly” (p. 16)—the mood cast here reappears in numerous passages (see, e.g., pp. 59, 61, 65). Whereas such comments may fleetingly if regularly annoy historians, I regret them more for their potentially longer lasting impression on general readers.

Issues about the book’s conceptual coherence (see below) prompt my specific worry, but both concerns share a common origin: Hirshfeld’s efforts to overcome his having “been schooled by scientists” (his phrase) and thus to write a flesh-and-blood *history* of parallax rather than a *science* of it (p. xiii). The tension between historian and scientist appears throughout the text, perhaps most succinctly in the summary listing of parallax values (p. 284). Hirshfeld’s stories take place prior to the detection of stellar parallax in the 1830s, but most of his tabulated values

(seven of thirteen) date from 1950 and later, and four of them are still to be determined.

Perhaps this criticism overinterprets an appendix table, but it and several other passages make me wonder about the book’s topic. Especially in the book’s first half, are all the stories, details, and characters coherently linked by efforts to determine a parallax? Is telling the history of such efforts really the book’s objective? For example, it includes lengthy (albeit interesting) digressions on the chemistry and physics of glass in early and later lenses and an entire chapter entitled “The Turbulent Lens,” a subject that, while intriguing, might not merit such fulsome attention here. A fair bit of introductory material strays beyond the topic so carefully defined, making it seem at times a gathering of interesting stories, facts, digressions, and anecdotes involving topics related to parallax, in contrast to the latter half of the book, which focuses more sharply on “the race” mentioned in the title.

Overall, Hirshfeld has done good research, finding the right sources (and consultants) to provide stories with new, accurate details. He rarely errs, though he relies on Vasco Ronchi’s discredited history of optics as the basis for “an exhaustive modern-day archival search” (p. 97) and makes an unfortunate though easily made error concerning an application of the inverse square law (p. 166) that may frustrate unwary readers. *Parallax* leaves room for other histories to flesh out more details, to incarnate its characters more fully, to locate them more completely in their contexts, and to allow readers to live alongside them more intimately. Even so, it succeeds in telling a story filled with the intrigue and drama that infused the search for this crucial and nearly intractable parameter.

MARVIN BOLT

Tom Shachtman. *Absolute Zero and the Conquest of Cold*. 261 pp., index. Boston: Houghton Mifflin, 1999. \$25.

Tom Shachtman’s book deals with the four-hundred-year history of the fascinating science of reaching low temperatures and producing cold. The book reads like a novel and recounts the history of reaching lower and lower temperatures and the discovery of new properties of matter when absolute zero is approached. At the same time, the efforts and adventures of the colorful characters who played a role in this story get full attention, together with the technological and commercial impact of their discoveries.

Shachtman begins his tale with Cornelis Drebbel’s attempts to “air-condition” Westminster

Abbey on a hot summer day in 1620 for the delight of King James I and continues with Robert Boyle, who embarked around 1665 on the scientific experimentation and inquiries that would lead to his well-known law. In 1787 Marinus van Marum liquefied ammonia, showing that Boyle's law did not hold in the new low-temperature region. With Daniel Fahrenheit (1720), the development of accurate thermometers began. The chapter "Through Heat to Cold" takes us into the early nineteenth century, where the steam engine powered the industrial revolution, and instructs us about the development of thermodynamics by unraveling the mechanisms through which heat can be converted into work (here he focuses on the work of Sadi Carnot [1824], William Thomson [1851], and Rudolf Clausius [1850–1860], with his famous fundamental theorem that the energy of the universe is constant and that entropy tends to a maximum). The conquest of cold came about through the study of heat. In 1873 Johannes Diderik van der Waals set out to develop a coherent description of real gases, taking into account the actual space occupied by real gas molecules, along with the forces they exert on one another, and to explain qualitatively the measurements of the isotherms of carbon dioxide performed by Thomas Andrews (1869). Shachtman describes in succession the liquefaction of oxygen (-183°C or 90°K) and nitrogen (-196°C or 77°K) in small droplets in 1877 by Raoul Pictet and by Louis Cailletet, the liquefaction of larger quantities using a cascade method by Syzgmunt Wroblewski and Karol Olszewski (1883) and by Heike Kamerlingh Onnes (1892), and the obtaining of patents in 1892 by both Carl Linde and William Hampson for large-scale gas-liquefaction processes based on Joule-Thomson expansion and the use of counterflow heat exchangers. In 1902 Georges Claude introduced the expansion piston at low temperatures, and soon both liquid oxygen and nitrogen were being produced on an industrial scale. In the meantime, James Dewar developed the glass thermos bottle with silver coating (1892) and in 1898 succeeded in reaching even lower temperatures with the first liquefaction of hydrogen (20°K above absolute zero). Dewar's apparatus produced only small amounts of liquid hydrogen. In 1906 his competitor Kamerlingh Onnes constructed a hydrogen liquefier capable of producing relatively large amounts. This steady supply was the key to his attempt to liquefy helium. On 10 July 1908 he accomplished the first liquefaction of helium at 4.2°K , and by reducing its pressure he reached 1.7°K above absolute zero the same day. This

fascinating period in the science of cold is illustrated with numerous details from the correspondence between Dewar and Onnes and others.

In the following years wonderful discoveries took place in the temperature range attainable by liquid helium. Onnes had the first glimpse of the strange new world of superfluidity when he discovered superconductivity in mercury in 1911. The superconductive state appears to be a remarkable example of a quantum state of macroscopic size. Roughly half a century of intensive research was necessary before an understanding of superconductivity on a microscopic basis was established by John Bardeen, Leon N. Cooper, and J. Robert Schrieffer (1957). Another mysterious phenomenon discovered at very low temperatures was the superfluidity in both helium isotopes: 4He below the lambda temperature (2.17°K) (Pyotr Kapitza and others [1938–1941]) and 3He in the milli-Kelvin range (Robert Richardson, David M. Lee, and Douglas Osheroff [1971]). The book ends by discussing laser cooling in the submicron Kelvin range near absolute zero with the creation of a new form of matter, a Bose-Einstein condensate (Carl E. Wieman, Eric A. Cornell, and Wolfgang Ketterle [1995]). The field of low-temperature physics has yielded twenty-one Nobel Prizes, an illustration of its scientific importance. All the principal cooling methods are discussed in the book except the very efficient 3He - 4He dilution refrigerators in the milli-Kelvin range.

RUDOLF DE BRUYN OUBOTER

Francis M. Carroll. *A Good and Wise Measure: The Struggle for the Canadian-American Border, 1783–1842.* xxi + 462 pp., notes, index. Toronto: University of Toronto Press, 2001. \$75, £50 (cloth); \$29.95, £20 (paper).

The area within a hundred miles on each side of the Canadian-American border is what human geographers call a "shatter belt." Language, customs, and mores all blend together. At the University of Maine a large percentage of the faculty, students, and staff are Canadian citizens or of Canadian heritage. No one can tell the Americans and the Canadians apart, as even the dialects of common speech are virtually identical, "aye" moving to "ayuh" and vice versa.

The writer of this review is a first-generation Yankee on the paternal side, and the author of the book was born and raised in Minnesota and teaches at the University of Manitoba. What the shatter belt will be like in the future is hard to tell. Today, with e-mail and television, the two entities seem to grow together, but the impact of

terrorist raids may now be driving them apart. Canadian nationalism and U.S. spread-eagles will continue to work changes. *A Good and Wise Measure* may help us understand the role of the past in determining the direction of the future.

“Undefended” borders and claims to space and resources did create some problems. In Maine and New Brunswick, for instance, little fracas broke out (and still do) over sovereignty. River drivers from the Maritimes crossed the border to destroy dams and release water on the Allagash and the St. John so that logs could move to the more lucrative market. These forays were of major significance as late as 1905, and the St. John River Commission (a joint body) still has some control over the river and boundary. The “undefended” and well-established border probably kept these squabbles to their low level.

Francis Carroll’s book provides a well-written and deeply researched account of the diplomatic history that established the border and the tracing out of that border by various surveying parties. This border creation was not a military affair, as were the surveying crews west of Lake of the Woods. Instead, it was local men, often relatives, determining the lines through triangulation, wading through the swamps, dragging chain, swatting insects, and subsisting on molasses, salt pork, and tea.

This book is more interested in treaties and talk than in men. The negotiations are well described, and Carroll is able to bring the discussions alive. We know who was involved and what it meant when we finish this book.

One may wish that we knew more about the “Main” John Glazier, who led one of the invasions from New Brunswick, about John Baker and his liberty poles, about the Indian Stream Republic and its declaration of independence from both countries—as well as such persons as “Stover” Rines, who put together a company of Bangor Tigers (loggers) in identical uniforms of heavy pants, plaid shirts, pea coats, and axes, marching them north at the time of the Aroostook War. Rines gave the name “rackereebos” to his home-grown militia, after a mythical beast whose distinguishing characteristic was that the nearer the animal sounded, the farther away it was.

What we do have is an excellent book on making the border, and then making it work. What could have been a difficult balkanized area became a shatter belt where arguments were small and individuals worked together, at least most of the time. The maps are excellent, and the research is first class.

Alexander Baring came to Maine when the border lands were purchased by William Bingham. He and Daniel Webster could see the long chance; the founders of the International Park at Campobello were able to do the same. This is an important book for all college libraries.

DAVID C. SMITH

Charles Officer; Jake Page. *A Fabulous Kingdom: The Exploration of the Arctic.* xii + 222 pp., illus., figs., refs., index. Oxford: Oxford University Press, 2001. \$25 (cloth).

The latest addition to the growing popular literature on polar exploration—the interest in which has produced three movies in the last few years about the British Antarctic explorer Ernest Shackleton and fills a shelf or two in the history section of many bookstores—comes from a professor of engineering at Dartmouth College and a science writer. In *A Fabulous Kingdom: The Exploration of the Arctic*, Charles Officer and Jake Page venture beyond the usual genre in this field of an individual explorer’s biography, setting out to write a history of the exploration of the Arctic from Pytheas’s voyage to Thule in 325 B.C. to David Fisher’s purchase of a round-trip ticket to the North Pole aboard a Russian nuclear-powered icebreaker in 1991. Along the way, many of the figures who have already received their own biographies appear, among them Fridtjof Nansen, Vilhjalmur Stephenson, Robert Falcon Scott, and John Rae, as well as lesser-known characters like Lincoln Ellsworth, the American who financed Roald Amundsen’s attempt to fly to the North Pole in 1925, and Semyon Ivanov Dezhnev, the Russian explorer who first discovered the Bering Strait in the 1640s.

Officer and Page’s book is briskly written, and the scientific portions are clearly explained. Any work that attempts to cover so long a time period must omit a great deal, and Officer and Page have sensibly decided to organize the book around the “three main goals” of Arctic exploration: the search for the Northeast Passage, the search for the Northwest Passage, and the attempt to reach the North Pole (p. xii). This means that more than two thirds of the book is devoted to nineteenth- and twentieth-century explorations, with only one chapter on ancient and medieval explorers and two chapters on the early modern period. The later chapters are generally better written and researched than the first few, and Officer and Page cite some primary sources for the twentieth century, especially focusing on those dealing with the Cook-Pearry priority claim

to the North Pole. But my general impression is that Oxford University Press was in too much of a hurry to add *A Fabulous Kingdom* to the bookstore shelves before the latest Shackleton movie had left the theater and did not apply its usual exacting standards. The book is poorly proofread, and names of major explorers are misspelled and inconsistently spelled (Bjarni Herjólfsson is called "Bjarni Herjulfsson," and Richard Byrd is "Richard Bird" on the book's jacket, for example). There are also factual errors and significant omissions: tenth-century Iceland was settled mainly by colonists from Norway, not Denmark (p. 26). The book's discussion of the study of Inuit customs begins with Franz Boas in the 1880s, although Danish writers had been reporting on Inuit life since Hans Egede in the 1740s. To dismiss these earlier accounts as mere "travel writing" (pp. 193–194) is not accurate, since the authors were living in Greenland among the Inuit.

Historians of science and technology might find portions of *A Fabulous Kingdom* useful for lower-division classes on the history of exploration. The book is not written for professional historians, however, and presents no new thesis or analysis of sources. Officer and Page's claim that in the "hard sciences—mainly physics and chemistry—it is pretty easy" to settle disputes, but in the "messy" areas of "Earth, biology, or human behavior, the scientific method is not so straightforward" (pp. 94–95), illustrates the general orientation of the book, which is "to show how myths arise and how they are—sometimes—dispelled" through scientific investigation (p. xi). Only occasionally do the authors attempt any critical analysis of the fascinating stories they tell—which is a pity, because there are a great many interesting vignettes in *A Fabulous Kingdom* that would have been worth closer study.

KAREN OSLUND

Peter L. Lutz. *The Rise of Experimental Biology: An Illustrated History.* Foreword by **Bob Boutilier.** xiv + 201 pp., illus., figs., bibl., index. Totowa, N.J.: Humana Press, 2002. \$59.50.

According to Peter L. Lutz, his primary impetus for writing *The Rise of Experimental Biology* was to satisfy the curiosity of life science and medical students about the "rich and sometimes amusing history of experimental biology" (p. 1). Lutz, a comparative physiologist, is McGinty Eminent Scholar in Marine Biology and Chair of the Department of Biological Sciences at Florida Atlantic University in Boca Raton. His re-

search in neurobiology and marine environmental physiology has resulted in numerous scientific articles as well as several books on the biology of sea turtles and survival mechanisms of anoxia-tolerant brains.

Although Lutz says that his interest in the history of medicine is long and deep, his purpose here is to entertain and inform the general reader while demonstrating how physiologists have used reason and experimentation to explain vital phenomena. Indeed, he notes that the literature is rich in detailed texts by specialists in the history of science and medicine. By combining narrative and illustrations, Lutz attempts to reflect the evolution of the biomedical sciences from Paleolithic cave art and trephination to eugenics and Nazi biology. In general, the book is composed of very brief biographical sketches of major figures in the history of science and skeletal outlines of history. Nevertheless, Lutz covers many major milestones, especially those that can be associated with interesting illustrations and anecdotes.

Lutz also attempts to call attention to the social context shaping the evolution of experimental biology, especially the abuse of science for political or economic ends. Perhaps another goal is to participate in the putative debate between scientists and postmodernist philosophers who argue that "all explanations represent opinions, and that all opinions are equally valid" (p. 2). Therefore, Lutz sees his narrative as another demonstration that the development of the scientific method is intrinsically different from other ways of knowing.

Candidly describing his selection of materials as rather idiosyncratic, Lutz notes that the text is a mixture of conventional keystones and items that he has found "amusing and particularly interesting" (p. vii). The text, therefore, meanders through history from prehistoric cave paintings to a brief chapter on contemporary science that is, unfortunately, devoid of illustrations. Readers of *Isis* are unlikely to discover anything new in either the narrative or the illustrations. The major or most photogenic contributions of Mesopotamia, Egypt, Greece, Rome, Islamic science, the European Middle Ages, the Renaissance, the Enlightenment, and the modern era are surveyed, with an emphasis on physiology. In keeping with his goal of providing a popular book for general reading, Lutz has kept bibliographical details to a minimum. A limited and rather outdated list of sources appears in the brief reference section at the end of the book.

As a miniature coffee-table book, *The Rise of Experimental Biology* offers a readable, acces-

sible narrative and interesting illustrations that should appeal to the general reader. Perhaps the book would make a good gift for high school or college students considering a career in the biomedical sciences. Even if they ignore the narrative, they might enjoy leafing through the illustrations and sharing bizarre bits of information at parties.

LOIS N. MAGNER

Alan Barnard. *History and Theory in Anthropology: Changing Perspectives*. xii + 243 pp., figs., tables, apps., bibl., index. Cambridge: Cambridge University Press, 2000. \$54.95.

An episodic account of the development of anthropological theories from the seventeenth century to the present, Alan Barnard's book derives from a lecture course he gave and is evidently intended for student readers. But it cannot serve as an introduction to its subject, since it assumes a good deal of knowledge. Barnard's text includes many disciplinary terms that he does not explain—though he may do so at last, if not at first, as when midway through the book he elaborates the concept of "relativism" that has already figured repeatedly in his classifications of theory types—and the definitions of terms provided in the book's glossary are often more allusive than informative. Barnard also presumes familiarity with anthropological literature; see, for example, his discussion of E. E. Evans-Pritchard's 1937 classic *Witchcraft, Oracles, and Magic among the Azande* (pp. 159–160), in which he provides a summary of a portion of Evans-Pritchard's narrative that only someone who has read it will recognize as an exemplary part of the whole. And Barnard makes statements that are surely comprehensible to members of his professional circle, but not to the rest of us, such as his reference to "an emotional variety of interpretivism almost unique to [Rodney] Needham's anthropology" (p. 136); evidently, Barnard disapproves of Needham's approach, but he should explain why. With the help of a good editor, Barnard might have made his book much more accessible.

But the book has defects other than exclusivity. It is replete with errors great and small. Because Barnard cites few references of any sort, one can only suspect that his errors indicate that he has not read many of the texts he discusses but has relied on secondary sources, as well as on memories of information acquired from lectures and conversations. For example, Barnard's representations of the ideas of Charles Darwin and Thomas Kuhn are simplistic in the extreme,

and he gets wrong various details of the ideas and careers of such figures as E. B. Tylor, J. G. Frazer, Sigmund Freud, Emile Durkheim, Max Weber, and Bronislaw Malinowski. Barnard also makes indefensible generalizations. In one of his book's few discussions of a branch of anthropology other than the sociocultural, for example, he speculates that in archaeology "universal evolutionism is perhaps a more natural theory than it is in cultural anthropology" (pp. 38–39), betraying his ignorance of archaeologists' long history of explaining cultural changes as products of diffusion through migrations of peoples. Perhaps the most egregious of his pronouncements is his assertion that "American anthropology began with the migration [to the United States] of Franz Boas, a German" (p. 54). This statement will confuse the reader: can it be reconciled with Barnard's account of the international influence of the American Lewis Henry Morgan (whose evolutionist scheme Boas repudiated)? Morgan did not make his living as an anthropologist, and by "began" Barnard probably means "was institutionalized as the basis of a full-time career," but he is apparently unaware that anthropology had been institutionalized in such loci as the Bureau of American Ethnology and Harvard University before Boas arrived on the American scene.

This is not to say that *History and Theory in Anthropology* is devoid of merit. In specific, its discussions of anthropological developments after the middle of the twentieth century are often extremely informative. But instructors who use the book in teaching should assign only those portions of it that will not mislead students.

HENRIKA KUKLICK

Stanley Finger. *Minds Behind the Brain: A History of the Pioneers and Their Discoveries*. xiv + 364 pp., illus., figs., index. New York: Oxford University Press, 2000. \$35.

This nearly four-hundred-page book covering the unraveling of the organization and functioning of the brain does not ignore the intertwined story of the nerves and neuromuscular system. It also touches on the human aspects of the great minds that made this work possible, thus making the story vibrant and more compelling. For example, Galen, who eschewed the pleasures of married life in his pursuit of the functions of the body and the treatment of its dysfunctions, ended up writing a textbook of medicine that was the principal reference for over a thousand years. His use of the electric torpedo ray fish in a strange way anticipated the electrical basis of the neu-

ronal net discovered a millennium and a half later. Stanley Finger's characters jump off the page "larger than life," and he artfully seduces us into being spectators at great discoveries and advances in knowledge.

This touched me personally, as one who began his first series of lectures at Columbia University nearly forty years ago by describing Galen's therapeutic use of the electric fish in treating headaches. Many times in subsequent chapters, whose subjects range from David Ferrier to Edgar Adrian and Henry Dale, I felt directly involved, and in fact I had either met or heard lectures by many of the luminaries mentioned in the later chapters.

At the beginning of the book, Finger's great erudition and diligent scholarship illuminate the little-known role of Imhotep from the Third Dynasty, Old Kingdom, in knowledge of the brain and neurosurgery from ancient Egypt; he then passes effortlessly to the accomplished physician Hippocrates, born to a Greek colonist in Asia Minor millennia later. Galen would call Hippocrates "the ideal physician" (p. 27); the subsequent systematic human dissection in the Hellenistic era, particularly by Herophilus in Alexandria, established in this "first age of enlightenment" the basis of the brain as the organ of mind, perhaps even the "dominant principle of the soul" (p. 35). Galen himself was also a colonist from the Greek territory of Pergamon in Asia Minor on the Ionian coast.

Although the codes of Roman society did not readily allow Galen to perform autopsies, his work with the great apes yielded meaningful insights—but also some errors that were not corrected until nearly twelve hundred years later by the human studies of Andreas Vesalius. *De humani corporis fabrica*—rescued from a garage sale giveaway!—stands on my shelves as a great illustrated poem of anatomy. Finger wonderfully describes Vesalius's very human life, with its many risks and tribulations. We are made to feel the impact of the bitter vituperation by his former teacher, Sylvius. The primacy of the pineal gland in the philosophical writings of Descartes is put in perspective, but it is with the description of the scientific method of Thomas Willis as the second age of enlightenment dawns that Finger really blossoms. Willis is also one of my heroes, and he, Ferrier, and Thomas Lewis form the trinity of another lecture I gave in 1979 at Columbia University in a symposium entitled "Vistas for the Neurosciences in the '80s." Willis is the epitome of a thoughtful and inquiring physician, a scientist who demanded proof and was showered with honors yet remained a gentle soul with "the

common touch" until his untimely death. Sir Charles Sherrington, who is also brought to life in a later chapter, said, "Thomas Willis of Oxford practically refounded anatomy and physiology of the brain and nerves" (p. 99). Interestingly, he made early and perhaps unique observations on the diagnosis and description of myasthenia gravis. In the way of the unbroken chain, Finger describes how a towering figure in the recent U.S. history of this disease, Henry Viets, then a fourth-year medical student, braved the U-boat dangers of the Atlantic near the end of World War I just to work with the great Sherrington.

One could go on to admire how vividly Luigi Galvani's serendipitous findings of "almost instantaneous" nerve conduction in the frog are described. They took nearly twenty years to get into print—unthinkable in the present climate of "publish or perish." The bitter controversy between Galvani and the powerful Alessandro Volta is portrayed; one must read Finger's book to savor the historical setting of this and of many other great sailors in the voyage of the mind. One can travel from the gothic horror of Mary Shelley's novel *Frankenstein: The Modern Prometheus* to the estimation of the speed of conduction by Hermann Helmholtz sixty years later. Paul Broca, John Hughlings Jackson, and Carl Wernicke are put in perspective, and the efforts of Charles-Edouard Brown-Séquard to educate the right hemisphere are mentioned. Even the connection with Robert Louis Stevenson's new *Zeitgeist* in *The Strange Case of Dr. Jekyll and Mr. Hyde* is made.

Ferrier, the great and controversial (to animal rights proponents) experimental clinician, starts the last 120 years. We find him at the Seventh International Medical Congress in London, with its medical experimental competitions, Crystal Palace reception, fireworks (including a portrait of Charcot), and more. The last chapters, on the almost-contemporary greats, are written in the same vibrant tone, and the same meticulous scholarship shows in the illustrations and chapter-organized footnotes and references. To mention one person, Sir Henry Dale of acetylcholine and neuromuscular fame—whom I actually heard lecture at the Royal Society of Medicine in the late 1940s—the infectious enthusiasm of this phenomenal basic scientist is accurately described. It was conveyed even when he lectured in his seventies, and his forsaking his teaching position at my old alma mater, University College and Medical School, to become the first director of the Wellcome physiological research laboratories may have pointed the way that a

number of my peers have taken in the second half of the twentieth century. Parenthetically, one of the brightest fellows who worked in my laboratories in the 1980s now occupies a similar position at Wellcome—of course, now within the current *Zeitgeist* of molecular genetics!

Finger has written a book that I will gladly put on my bookshelf alongside Galvani's *De viribus electricitatus in motu musculari commentarius* (or I would if I had it!), Sherrington's *The Integrative Action of the Nervous System*, J. Z. Young and Tom Margerison's *From Molecule to Man: The Explosion of Science*, and the wonderful illustrated *Medicine: A Treasury of Art and Literature* edited by Ann G. Carmichael and Richard M. Ratzan. It would be caviling to say that I miss the early years of Adrian at the National Hospital Queen Square, the landmark studies of J. Z. Young on the double brain octopus and memory, and the terrible controversy between the pre- and postjunctional theories involving Nobel laureates in the 1960s. In fact, this book calls out for a miniseries, perhaps on public television and extending even to eight or ten hours. We might invoke the help of the medical literati—say, Oliver Sacks as an advisor/medical discoverer. I'm sure Henry Dale would have approved with enthusiasm!

ROBERT E. LOVELACE

■ Antiquity

Charles Frankel. *The End of the Dinosaurs: Chicxulub Crater and Mass Extinctions*. xii + 223 pp., illus., tables, bibl., index. 1996. Cambridge: Cambridge University Press, 1999. \$24.95.

The belief that the dinosaurs were destroyed by a giant careening meteorite is not original, having been advanced many times in the postwar years. Prior to 1980, however, most geologists were still uniformitarians and hostile to simplistic messages of cosmic theories that seemed to cut through decades of paleontological work. Also, as Charles Frankel points out, there were generational issues: most earth scientists were trained in the 1950s and 1960s.

But in 1980 the Berkeley father and son scientists Luis and Walter Alvarez, along with other luminaries like Frank Asaro, found mineral evidence that a giant bolide had crashed to earth some 65 million years ago. This discovery was the beginning of respectability for catastrophism.

The standard rebuttal of the "impact extinction" thesis was the volcanism argument.

Charles Officer and Charles Drake had argued that millions of tons of volcanic ash and gases, spewed out over hundreds of thousands of years, had fundamentally altered the dinosaurs' climate. This thesis was supported by other influential earth scientists, among them Vincent Courtillot.

The prevailing orthodoxy was finally overturned because of what was known about earth processes and the discovery of minerals that were clearly and irrefutably of extraterrestrial origin. It was not just the presence of the renowned iridium at the K-T geological boundary, but also the finding of osmium and palladium—not at all the kinds of metals a volcano would produce in such vast amounts.

In 1984 Bruce Bohor and colleagues from the U.S. Geological Survey reported the discovery of shocked quartz at K-T sites in Montana. This in turn produced a flurry of similar discoveries at other, widely separated, K-T sites. This evidence, even by itself, would have clinched the argument, as shocked quartz could have arisen in the rock strata only as the result of a violent prehistoric impact.

With regard to the crater impact site, the evidence was not at all clear as to whether the giant bolide had landed in the ocean or on land. Many crater sites were not right in size or age. A prime suspect, the Manson crater in Iowa, was thought to be the target area, but improved argon dating techniques proved it to be too old.

Frankel highlights two other intriguing pieces of decisive detective work. One was the discovery of evidence of a great deal of tsunami flooding in the Gulf of Mexico area by Joanne Bourgeois and colleagues from Washington University. The other was the serendipitous reporting of a massive circular feature in the Yucatan area (hinting at sizable oil or gas deposits) at an obscure geological meeting run by Pemex, Mexico's national oil company, by a member of the press. This turned out to be the site of the famous Chicxulub crater, one of the largest on earth and, impressively, the size suggested by the Alvarizes in 1980—about 180 kilometers in diameter.

Somewhat surprising is Frankel's reversal of the consensus on the climatic aftermath of the event. It was generally thought that raging forest fires took place, followed by a prolonged cooling as the dust and meteoritic grains darkened the skies for years, severely curtailing photosynthesis and bringing about a collapse of the food chain. Citing the sedimentary evidence, the author points instead to a postimpact cooling lasting "months to years," with a subsequent warm-

ing that lasted for centuries or “thousands” of years.

Ironically, catastrophism has never had much of a following in the United States, partly because of the debates between creationists and evolutionists and also because of the cultish influence of Immanuel Velikovsky, a pseudo-scientist who believed that ancient myths could be explained by a near-collision between the inner planets and earth. What is commendable about Frankel’s illustrated book is the quiet, step-by-step manner in which he shows how the catastrophic explanation scientifically and empirically defeated the “gradualist” one. At a symposium in 1994 not a single geologist contested the impact thesis nor disputed the date or size or location of the impact site.

ANTONY MILNE

Anthony F. Aveni. *Skywatchers: A Revised and Updated Version of Skywatchers of Ancient Mexico*. 423 pp., figs., tables, notes, bibl., index. Austin: University of Texas Press, 2001. \$75 (cloth); \$34.95 (paper).

Honored repeatedly as a teacher at his own university (Colgate), Anthony Aveni is widely respected for his interest in conjoining the two worlds of hard science, in the form of astronomy and physics, and of culture more broadly. In just this capacity, he has since the 1970s been one of the most distinguished champions of archaeoastronomy, that is, of studying traditions of knowledge with regard to the sky that are distant from us in time or space and whose credentials as science proper have been little valued.

When it appeared more than twenty years ago, *Skywatchers of Ancient Mexico* was heralded as a truly pioneering work, which responsibly gathered between two covers the main advances in Western understanding of Mesoamerican accounts of the sky, especially those of the lowland Maya. As Aveni points out in the introduction, there have been many advances since that time, thanks in particular to the ongoing decipherment of Mayan hieroglyphic texts. These have been incorporated and assessed in the revised edition.

The basic structure and argument of the work remain much the same. After the introduction (Ch. 1), we learn successively about the cultural context in which astronomy was practiced in Native America, particularly Mexico and its middle part (Ch. 2); the possibilities and limitations of naked-eye observation (Ch. 3); questions of mathematics and calendrics (Ch. 4); and, finally, Aveni’s forte: the role of astronomy with respect to architectural design and urban planning

(Ch. 5). Where appropriate, figures and tables are updated, while the existence of modern software renders obsolete former reliance on magnetic cards.

A welcome feature of Aveni’s work is its concern to engage the reader directly, “unencumbered by disciplinary jargon and as free of complex prerequisites as I could manage,” as he says in the postscript (p. 339). He also takes pains to gauge cultural differences that separate ourselves and his subject, indicating how unproductive it can be always to be asking how “scientific” Mesoamerican astronomy is, according to entirely Western preferences in methodology and aim. In all this he shows an admirable openness, broaching questions that more staid colleagues would never even allude to, and never forgetting that many of the astronomical achievements the West prides itself on in fact came quite late in the day, and certainly after Columbus. The precession of the equinoxes is such a question. Rather than dismissing out of hand the notion that Mesoamericans could ever have been near to understanding precession, he calmly produces and assesses such evidence as there is. His perspective here is sensitive to the idea that it would be rather surprising if Mesoamericans had not wondered about the effects of precession, given how long their calendar had been in continuous use.

Generally speaking, the exposition and documentation are excellent, clear and professional. There are, however, one or two less impressive moments. Perhaps the weakest strands of his argument concern the predecessors of the Maya—the Olmec, about whom we told quite little—and the *altiplano* tradition exemplified by the Aztecs at the time Cortés arrived. The 260-night cycle, which Aveni deems “mysterious,” was in fact shown by Schultze Jena, in the 1930s, to be lunar and relevant to childbirth. Similarly, the belief in “layers” of sky and underworld he confidently ascribes to this tradition has been severely questioned by others; and indeed the first valency of such a model would seem to have to do less with material Ptolomaic levels than with the kind of intricate and astoundingly fertile cult of numbered time cycles, of the type Otto Neugebauer analyzed so keenly in *History of Ancient Mathematical Astronomy*. Again, at times the quality of the figures militates against clear understanding. The account of the heavens recorded in the Tepepulco Manuscript and copied by Friar Bernardino de Sahagún, which sophisticatedly alternates moving bodies with stars and constellations, is still reproduced in the seriously defective Paso y Troncoso version of over a century ago, when in fact in the meantime a mag-

nificent facsimile in full color has become available.

Like Tony Aveni's other work, this updated account of America's ancient skies should be welcomed with open arms: it is readable and very well informed and will constantly serve anyone at all concerned with that subject.

GORDON BROTHERSTON

John North. *Stonehenge: A New Interpretation of Prehistoric Man and the Cosmos.* xlv + 609 pp., illus., tables, apps., bibl., index. New York/London: Free Press, 1996. \$35.

This long, imaginative, and highly original book returns to a subject that caught much attention a generation ago: the way Stonehenge is (or is not) so organized as to make some kind of astronom-

ical observatory or calculating machine. Stonehenge, most famous of the megalithic structures of later prehistoric Europe, is a set of shaped stone blocks set either vertically or as horizontal lintels bridging between the verticals, the whole on a symmetrical axis aligned northeast to southwest.

A central difficulty with earlier studies on Stonehenge, such as Gerald Hawkins's successful *Stonehenge Decoded* (Dell, 1965), is that they were anecdotal, accounts particular to this singular monument, rather than inclusive of the many varied structures in earth and wood as well as stone of its Neolithic age in Britain and Europe. John North also addresses a great range of other sites, only reaching Stonehenge halfway through his book. He finds in them a consistent pattern, and one that largely involves stars,



Stonehenge, engraved for William Camden's 1695 book *Britannia* (from North, *Stonehenge*, p. 437).

where previous astronomies have largely concerned the sun and moon.

Another important advance is that North considers the three-dimensional properties not just of Stonehenge but of other sites where earthen banks and mounds make artificial horizons and where one conjectures there were wooden post-and-beam settings, structures that survive archaeologically only in the postholes that were their foundation supports. Studies of complex monuments from their plans alone reveal a great many alignments among their features, some of which will by chance coincide with the myriad points on the horizon where the sun or the moon or a planet or a star rises or sets at some time in its complex movements as those varied across the centuries. Coincidence of *some* alignments with *some* astronomical orientations means little; projected beyond the horizon, the axis of Stonehenge is said to pass through the center of Copenhagen—a fact that, if true, says nothing about the placing of either Stonehenge or Copenhagen. Accordingly, the best proof we have of astronomy in the ancient British Isles is in the long entrance passage to the chambered mound at Newgrange (Ireland), broadly of the era of Stonehenge. There, a “roof-box” was built into the cover of the sloping passage, such that at the midwinter solstice the rising sun illuminates the central chamber. That is, here a distinctive architectural element was built that requires correct placing in three dimensions; it therefore offers a proof an order of magnitude better than notice of a two-dimensional alignment.

Working in this kind of way, and benefiting from a historian of science’s grasp of earlier weaknesses in studying Stonehenge, North especially addresses the earthen monuments and timber settings. Among the earthen monuments are the “long barrows,” mounds typically tens of meters long paralleled by matching quarry ditches from which their chalk mass was dug; they often contain human burials at one end but are too large simply to be graves in a functional sense. Here the long horizontal profiles of the mounds are seen as making artificial horizons, to be used by astronomers standing in their ditches. Among the wooden monuments are the several timber settings, known as “hengés” because they resemble Stonehenge without the stone, that survive only in plan form. Still, those surviving plans show from the size of the post-molds how thick the posts were; reckoning that the thicker a post, the higher it stood above ground, and using the example of Stonehenge to surmise that the vertical posts will have supported horizontal lintels, North can reconstruct

the wooden monument in three dimensions and explore its astronomical potential.

All this is done with great thoroughness and care and with some new observations. The sometimes obscure archaeological reports are well understood, and the work is illustrated with a great many elegant figures. It does not persuade me; I am uneasy with the degree to which those reconstructions are conjectural—and uneasy with the society they suppose to have existed in prehistoric Europe. I still very much doubt so much knowledge of astronomical regularities would exist in a society without writing and of that time and nature.

On the face of it, the method in all these studies of a prehistoric astronomy is straightforward enough: if ancient people incorporated in their built structures two- and three-dimensional alignments to heavenly bodies, then by studying the built structures we can recover the alignments and the ancient knowledge they expressed. John North’s book, like its many predecessors, shows that a better method is required. May it come to exist.

CHRISTOPHER CHIPPINDALE

Plato. *Timaeus*. Translated by **Donald J. Zeyl**. xcvi + 94 pp., figs., bibl. Indianapolis: Hackett Publishing Company, 2000. \$29.95 (cloth); \$10.95 (paper).

This is a most useful addition to the available translations of the works of Plato, but it is also a work that should be of particular interest to historians of science. Plato’s *Timaeus* is an attempt to present a cosmogony and anthropology, in the context of providing a background to a portrayal of the working of an ideal state—a project that was never brought to completion as such (the companion dialogue to the *Timaeus*, the *Critias*, breaks off in mid-sentence) but that finds a sort of complement in Plato’s last work, *The Laws*.

The discourse is put by Plato into the mouth of a visiting Pythagorean sage, Timaeus of Locri—a move that led the eminent classical scholar A. E. Taylor to propose that Plato is here not presenting his own views at all, but actually those of a fifth-century Pythagorean. While Taylor is surely wrong about this, the device enables Plato to distance himself to some extent from the doctrines proposed, as does the repeated warning he puts into Timaeus’s mouth to the effect that the views expressed are only a “likely account” (*eikôs logos*). The fact is that Plato has a relatively low view of the degree of accuracy that

can be attained in any inquiry concerning the physical world.

However, he did plainly, in his later years, take a considerable interest in how and why the world and mankind were created, and this remarkable work is the fruit of those speculations. Irrespective of how seriously he intended the details to be taken, they were taken very seriously by philosophers and scientists in late antiquity, when the *Timaeus* took on the status almost of a handbook or encyclopedia. Interest in it in modern times has been considerably less, to the extent that many classical philosophers in the Anglo-American tradition (taking their cue here, to some extent, from such a figure as G. E. L. Owen) have been inclined to ignore it.

But there is much of interest in this work, and its importance in the history of Platonism is very great. Classical philosophers tend to concentrate rather on the first, more cosmological, two-thirds of the work, from 27D to about 56C, or perhaps 69A (leaving aside the introductory conversation, from 17A to 27D), but for historians of science (and in particular of medicine and physiology) the last third, from 69A to 92C, which chiefly concerns the composition of the human body, is really of more interest. Here Plato, though not an expert himself, seems to be relying on the best contemporary medical theorizing, so his views show interesting connections with various treatises of the Hippocratic corpus.

Donald Zeyl has produced an excellently clear and accurate translation (previously published, in fact, in a collection of Plato's *Complete Works*, edited by John M. Cooper and D. S. Hutchinson [Hackett, 1997]) and also a lengthy (eighty-nine pages) introductory essay, which dwells on all the issues raised in the work and takes sensible stances on various disputed questions (e.g., he lays to rest a troublesome hare raised by Harold Cherniss about the real subject matter of *Timaeus* 49C–50B). On the large question of whether the account of the temporal creation of the world is to be taken literally, he declines to commit himself, though stating the arguments on both sides—but that is reasonable in a work of this sort. The book is rounded off with a useful bibliography.

JOHN DILLON

Theodor S. Jacobsen. *Planetary Systems from the Ancient Greeks to Kepler.* xvi + 256 pp., illus., tables, bibl. Seattle: University of Washington Press, 1999.

That this volume has appeared is a labor of love by colleagues on behalf of an elderly professor

who spent years working through the technical details of ancient to Renaissance celestial mechanics. Armed with a solid command of languages, he produced a guide that might have been particularly helpful in the days before Gerald Toomer's definitive translation of the *Almagest*, O. Neugebauer's *History of Ancient Mathematical Astronomy*, William Donahue's English version of Kepler's *Astronomia nova*, or N. M. Swerdlow and Neugebauer's treatise on the mathematical astronomy of Copernicus. None of these works figure in the quaintly dated bibliography that assisted the present work.

What we have here are tidy diagrams, modern equations and accurate geometry, old-fashioned historiography supported by strong opinions (such as how queer Plato was, with his "orgy of anthropomorphic, metaphysical, ethical, and aesthetic judgements that are scientifically irrelevant" [p. 29]), and essentially no significant insights. The book was obviously not written for historians of science, and to the professionals it can only be considered an embarrassment. Yet Jacobsen does say, apropos of an earlier opinion that Kepler's reputation would have been higher if he had burned three quarters of his work, that "this remark reveals a deplorable ahistorical viewpoint and reminds one of the narrow judgement of some of the French encyclopedists, who, intoxicated with perspectives gained during the Age of Reason, . . . exhibited the smallness of mind to accuse Kepler of being a 'scatterbrained mystic'" (p. 172).

There are, very occasionally, small errors in the text, such as the statement that Hipparchus had discovered the effect of evection in the lunar motion through the timing of eclipses; since this is an anomaly absent at syzygies, the claim must be wrong. Almost universally Ptolemy is credited with the discovery of evection. Furthermore, Ptolemy found its amplitude to be right on the modern value, contrary to the value Jacobsen gives. One must assume, however, that the numerous equations that appear throughout his book are correct, as it would be a thankless task to prove otherwise.

OWEN GINGERICH

Adrienne Mayor. *The First Fossil Hunters: Paleontology in Greek and Roman Times.* xx + 361 pp., illus., figs., tables, bibl., index. Princeton, N.J.: Princeton University Press, 2000. \$35.

Griffins and giants are standard figures in ancient Greek mythology and art. The latter were referred to as early as Homer's *Odyssey* 7.59. In the original meaning of the Greek word for "gi-

ant,” they were basically thought of as beings of great strength and monstrous size and appearance (e.g., Titans, Cyclops, centaurs), engendered by the divine personification of primeval powers and eventually defeated by the Olympian gods after a long and dreadful fight (gigantomachy). Subsequently, mythic heroes, famous (Theseus, Pelops, Ajax, and Orestes) and obscure, were conceived as gigantic in size. As for griffins, first and suspiciously alluded to by Herodotus (*Histories* 3.119), they were said to be guardians of gold mines in remote northern areas later identified as the Altai Mountains and the Gobi Desert. They were traditionally depicted as four-footed, crooked-beaked, winged creatures. Until recently, historians of ancient natural science had little, if anything, to do with such fabulous beings, which were noticeably absent from the surviving writings of Aristotle and the other philosophers interested in nature and animals.

Adrienne Mayor’s thought-provoking book will mark a watershed in the approach to griffins and giants. As a classical folklorist investigating “the historical and scientific realities embedded in the Greek and Roman myths,” she has expertly pieced together an impressive array of textual and iconographic evidence on those so-called fancies and crossed it with paleontological findings dating back to the Miocene, Pliocene, and Pleistocene epochs in mainland and insular Greece and to the Mesozoic era in Central Asia. Strikingly enough, many of these findings were unearthed from sites where legendary accounts indeed located griffins and giants. Through her thorough and penetrating analysis, Mayor convincingly argues that the griffins described in Scythian tales were inspired by the skeletons of *Protoceratops andrewsi* and other dinosaurs that littered the Gobi Desert by the end of the Cretaceous period and “are still continuously revealed by the very same forces of erosion that bring the gold down from the mountain” (p. 43). As to the pre-Olympian giants and their later heroic counterparts, Mayor suggests that they originated from the early stories woven to explain the fossilized big bones and tusks embedded in many places around the Aegean and other parts of the Mediterranean Sea and duly collected over time, measured, and displayed in temples and museums. In other words, the classical tradition of writing about giants conveyed the ancient Greeks’ and Romans’ curiosity about petrified remains and their intuitions about gigantic creatures “that were destroyed by catastrophe or died out long before current human beings appeared on the earth” (p. 8).

This original and ingenious insight is under-

standably speculative to some extent. In places, one might even express reservations regarding some of the author’s one-sided views. For example, no attention is paid to the Oriental origin of or influence on Homer’s and Hesiod’s narratives about the Titans and other “geomorphic” monsters. Moreover, translating the Greek “*drakon*” as “dragon” in Philostratus’s *Vita of Apollonius of Tyana* (against Jaap-Jan Flinterman, who translated it as “snake”; see p. 305 n 27) or in Palaephatus’s *On Unbelievable Tales* sounds as anachronistic and misleading as does the commonplace view of Aristotle’s so-called fixity of species, which Mayor rightly criticizes (see pp. 8, 217–218). Providing scales would have made the seven maps, which are most welcome, even more effective. The cross-reference on page 136 should read “Chapter 4” rather than “Chapter 5” (it is correct on p. 306).

Yet these criticisms are trifles in the context of the overall picture, which is as fascinating as it is persuasive. Mayor has made a key contribution to the understanding of unsuspected aspects of ancient peoples’ interest in paleontological matters. Furthermore, her sixth and final chapter ends with challenging parallels between ancient and modern paleontological “fictions” and is relevant to the long-range history of mentalities. For both its innovative method and its results, this well-balanced and vividly written book belongs on the bookshelf of every historian of natural sciences.

LILIANE BODSON

Franjo Kovačić. *Der Begriff der Physis bei Galen vor dem Hintergrund seiner Vorgänger.* (Philosophie der Antike, 12.) 320 pp., bibl., indexes. Stuttgart: Franz Steiner Verlag, 2001. €70 (cloth).

This careful and well-structured study of the meaning of “*physis*” in Galen looks at one of the central concepts of his medical philosophy against the background of his relationship with earlier philosophers. Three main meanings are distinguished, although often Galen slides easily from one to another. “Nature” may stand for a transcendental, godlike, and providential Demiurge, deriving from Plato via Aristotle and Stoicism. It is also the system of rules and organization that governs the species, as well as that which is immanent in the individual. How all three fit together can best be seen in embryology, where the specific nature of the individual is passed on through the larger controlling force of the species, mediating the foresight of the Demiurge.

If none of this is entirely new—as is shown by the opening chapters, which set out the views of other scholars over the last two hundred years—this book will prove a very valuable guide through some difficult material. The exposition of the opinions of Galen and of later scholars is both clear and judicious, and all debts are scrupulously acknowledged. Franjo Kovačić is at his best and most eloquent when discussing Galen's theology and what some have seen as his deification of Nature, concluding that Galen was an "innovative theologian." His argument that for Galen the traditional gods were personal manifestations of the divine, whose highest, unpersonal, form is Nature, is plausible but needs more justification in the light of contemporary beliefs.

But some wider questions are ignored. It would have been useful to test Galen's claim that he rarely changed his mind on any significant issue by looking at the ways in which he deploys his vocabulary of nature. Sometimes Galen's shifts in emphasis are determined by his projected audience or opponents; at others points his very subject matter may influence his choice of words.

Errors and misprints are few. But while acknowledging that both the commentary on *Nutriments* and that on *Humours* are not by Galen, even if they contain some recycled genuine material, Kovačić nonetheless continues to cite them as an "interpretation" of Galen's views, a procedure that is either illegitimate, where no other evidence survives, or redundant, where it does and may have supplied the material for the Renaissance forger. The fragment on diagnosis from dreams is marked as "pseudo-Galen" (pp. 171, 244) but then cited as if genuine (which it is). The unlucky timing of this dissertation means that, although some references could be added to my 1999 edition of Galen's *De propriis placitis* (Akademie Verlag), Kovačić could not fully use the new material it offers on Galen's doctrines of the soul and on his embryology. This is a pity, for Kovačić is intelligent and fair-minded, and his interpretation of these new and complex problems would have been welcome.

VIVIAN NUTTON

Owsei Temkin. *"On Second Thought" and Other Essays in the History of Medicine and Science.* x + 272 pp., index. Baltimore/London: Johns Hopkins University Press, 2002. \$42 (cloth).

In the year of his one hundredth birthday—which, sadly, turned out to be the year of his

death as well—Owsei Temkin published a collection of his essays. These reprinted pieces, which originally appeared in various journals and scholarly collections between 1948 and 1981, are gathered here under such broad thematic headings as "Ethics in Medicine," "The History of Science," "The History of Therapy and Nutrition," and "A Miscellany." The volume thus offers readers a welcome opportunity to retrace, in concentrated form, the myriad qualities of Temkin's thought that, taken together, have rightly secured him a place as one of the world's leading medical historians. These include his encyclopaedic erudition, which, singularly, allowed him to conceive of the history of medicine as an integral discipline, thus enabling him to mediate like no other between medicine and history, pharmaceutical history and the history of science, the history of ideas, of culture, of religion, and of philosophy; his linguistic expertise, which informed his lifelong concern with the autoptic study of every source text available to researchers of Western medical history; his distinguished academic career, which, spanning more than seven decades, not only saw him affiliated with such leading institutions in the field of medical history as Leipzig during the Weimar Republic and Johns Hopkins after 1933, but also allowed him to develop a uniquely broad perspective both on the current state of research at any given time and on the history of the discipline as such, which in turn immunized him against any form of methodological dogma; and, last but not least, his skills as a writer and storyteller whose erudite and thoughtful works are as instructive to read as they are pleasurable and entertaining. Notably, however, the current volume does not merely comprise reprints—a sort of second volume of Temkin's "minor works," a sequel to *The Double Face of Janus* (Johns Hopkins, 1977)—but also contains two previously unpublished pieces. The first of these (pp. 1–18) lends the volume its main title and serves as an introduction to what is to follow; largely personal in tone, it takes the reader on a reflective journey across such seemingly disparate fields as biography and historiography, antiquity and modernity, life and work. The second piece, bearing the deceptively simple title "What Does the Hippocratic Oath Say?" (pp. 21–28), presents readers with a new English translation of the famous text combined with an interpretative commentary based on—or retrospectively motivating?—this new translation. With his new interpretation of the oath, Temkin departs from Ludwig Edelstein's frequently invoked 1943 hypothesis, according to which the oath has to be

read against the backdrop of neo-Pythagorean teachings and thus constitutes a post- or pseudo-Hippocratic and esoteric text; Edelstein's hypothesis further interprets the pronouncements on abortion as a categorical ban on abortion, the passage on the deadly drug as a ban on physician-assisted suicide, and the passage on cutting the stone as a categorical ban on surgical intervention. The decidedly hypothetical character of Edelstein's reading has been pointed out from a number of different perspectives by classicists such as Fridolf Kudlien, Jutta Kollesch, and Heinrich von Staden. Temkin's contribution raises fresh hopes for a revitalization of the debate surrounding the Hippocratic oath in medico-ethical circles as well. There, Edelstein's reading has very much reigned as the accepted *opinio communis*, on the basis of which it seemed safe to turn one's attention once and for all to an unproblematically constructed post-Hippocratic era. It should be hoped that Temkin's article will reopen a case that, post Edelstein, had been very much regarded as closed. For one thing, the instrumentalization of Edelstein's reading on the part of medical ethics not only contravenes Edelstein's own intentions; as Temkin's work illustrates, it also constitutes a factual error. Temkin's article thus stands as a powerful testimony to scholarly integrity, an integrity that values independent thought above party-line parroting, the possibility of revising one's opinion above the scholarly decree, the love of truth above the cult of friendship, and an appreciation of the problems and difficulties involved in any scholarly endeavor above the short-term satisfaction gained from easy answers. What is more, Temkin speaks to the reader in a uniquely personal voice that productively blurs the boundaries between life and work, man and scholar—a voice, in short, that speaks of an authenticity that few medical historians, particularly of Temkin's generation, have managed to preserve.

THOMAS RÜTTEN

Hippocrates. *On Head Wounds*. Edited and translated with commentaries by **Maury Hanson**. (Corpus Medicorum Graecorum, I.4.1.) 130 pp., bibl., index. Berlin: Akademie Verlag, 1999.

This new edition of the Hippocratic treatise *On Head Wounds* [*De capitis vulneribus*] is splendid, for Maury Hanson, the editor, is equally capable as a textual critic and as a medical doctor with experience in assessment and management of cranial injuries. Every aspect of the treatise has been meticulously examined afresh, and there is much here to interest a wide audience,

from classicists pursuing the language and argumentation of early Greek prose to historians of science concerned with surgical techniques employed at the end of the fifth century B.C.—and anyone interested in how an early Greek practitioner coped with serious head injuries.

The Hippocratic author begins by describing the skull, with its sutures and diploe, well aware of the varying degrees of thickness of the bone and the interrelationship between the seriousness of the injury and a patient's chance for survival (Chs. 1–3). His typology for head injuries includes cracklike fractures (*rhomai*), crush-in injuries (*phlaseis*), and depressed fractures (*esphlaseis*), all of which were sometimes complicated by a *hedre*, the depression or hole made by a weapon's impact (Chs. 4–8). Indications for trephination initiate the medical writer's clinical evaluations (Chs. 9–12), although techniques for trephining are given only in the final chapter (Ch. 21). The patient's wound was to be examined to determine whether it occurred in the stronger or weaker part of the skull, whether the hair at the site was driven into the wound, and whether the bone had been stripped bare. The physician also gathered as much information as possible about the circumstances under which the patient sustained the injury, including his initial reaction to the blow; such information was especially helpful when the fracture was difficult to see. Chapters 13–20 set forth methods for treatment: wounds on the face required poulticing and bandaging, but those elsewhere on the head were to be kept dry, unless widened through incision. The medical writer explains how he used various surgical instruments—probe (*mele*), rasp (*xyster*), perforator or auger (*trypanon*), trephine (*prion*), and scalpel (the latter indicated only by words meaning "cut")—as well as a black solution to clarify a fracture's position. Because children had thinner and softer bones, he preferred to use the perforator rather than the trephine for them. The attending physician needed to know the signs that the case was not proceeding well so as to predict a patient's death (*prolegein*): fever that attacked within fourteen days in winter, seven in summer; suppuration of the wound and discoloration of the bone; delirium and convulsions. Not being the first to attend a patient meant that one was not only behind schedule with medical treatments but perhaps would have to correct a predecessor's errors as well.

Hanson's Greek text is based on a re-examination of the nine Byzantine manuscripts that contain *Head Wounds*, deriving most readings from the earliest, MS Laur. Gr. 74,7, s. X, and

MS Vat. Gr. 276, s. XII; the indirect tradition is traced through Erotian's dictionary of Hippocratic terms, composed in the reign of Nero, and Galen's glossary. Galen wrote a commentary to *Head Wounds* that, although it was known to Oribasius in the mid-fourth century, has not survived. Hanson's English translation well represents the Hippocratic writer's repetitive yet spare Greek; he avoids anachronisms that thrust modern vocabulary and concepts into the context of ancient surgery. At the same time, however, Hanson's commentary provides a treasury of philological detail, juxtaposed to current medical opinions on the many topics the Hippocratic writer introduced: for example, Hanson notes that though the author had an appreciation of the importance of the brain, he paid scant attention to the neurologic complications now associated with head trauma, apart from brief mentions of impaired consciousness and contralateral seizures. While the Hippocratic author claimed that he poulticed and bandaged the face to prevent swelling caused by an influx of blood and that he did not do so elsewhere because the scalp lacked an abundant supply of blood, Hanson explains that facial swelling is due to the structure of facial skin, with blood vessels rupturing in the case of a closed wound and with intercellular liquid seeping out owing to damage in cell membranes in the case of an open one; further, the blood supply to the scalp is rich. Hanson's respect for the Hippocratic author is often apparent; thus, here, he underscores that the writer was correct in his view that scalp and skin respond differently to trauma and notes that the variation in his treatments may well have produced the desired result.

ANN ELLIS HANSON

■ Middle Ages and Renaissance

Petrus Peregrinus de Maricourt. *Opera: Epistula de magnete; Nova compositio astrolabii particularis.* Edited by **Loris Sturlese** and **Ron B. Thomson.** (Centro di Cultura Medievale, 5.) 208 pp., illus., figs., tables. Pisa: Scuola Normale Superiore, 1995. L 60,000.

Petrus Peregrinus has been known to posterity by the two treatises edited here (especially the first) and by various encomia from Roger Bacon, of which the following is a sample: "He does not care about speeches and verbal battles, but pursues wisdom's works, and is content with them. . . . He is a master of experiments, and so he knows natural, medicinal, and alchemical things by experience. . . . He is ashamed if any layman,

or old woman, or soldier, or rustic from the fields should know anything that he does not. Therefore he investigated all works of founding metals, and all things that are worked in gold, silver, other metals and all minerals; and he knew all that pertained to warfare, to arms, and to hunting; and examined everything pertaining to agriculture, the measurement of fields, and to rural works; he considered the experiments, divinations, and charms of old women and all magicians, and similarly the illusions and devices of all jugglers." Bacon's enthusiasm spilled over into imitation to such an extent that William Gilbert could report in his *De magnete* (probably with much approbation) the opinion that Peter's work on magnetism was dependent on Bacon's ideas. This is obviously incorrect, but Peter's lucidly presented and intelligently argued *Letter on the Lodestone* made him a hero of experimental science to earlier generations of historians, even if it did end up with a design for a perpetual motion machine. That his skills extended to mathematics is evinced both by Bacon's testimonies and by his own *Nova compositio astrolabii particularis*, on which Ron Thomson quotes Emmanuel Poulle: "par sa clarté, sa précision, son souci d'être complet sans prolixité, il est une des meilleures productions de ce genre au Moyen Age."

Medieval scientific heroes have been out of fashion for several decades, but, while producing numerous valuable insights, the antianachronistic drive that brought this about has resulted in a chasm between the historiography of medieval natural philosophy and that of medieval technology. The present lean but very valuable edition of Peter's two extant works (which also incorporates a Renaissance Italian translation of most of the *De magnete*) prompts one to ask whether this is just. We have virtually no biographical information on Peter, but he probably earned his living as a kind of military engineer. His writings make it clear that he was by no means afraid of getting his hands dirty, but also that he applied an incisive theoretical intellect to his practical doings. It is implausible to suppose that he was unique in this, and recent work on medieval alchemy has highlighted another domain for finding fruitful symbioses of theory and practice. Historians of medieval science should perhaps assert more vigorously that at least for part of their time they are actually dealing with *science*.

GEORGE MOLLAND

Albertus Magnus. *On Animals: A Medieval Summa Zoologica.* Translated and annotated by

Kenneth F. Kittell, Jr., and Irven Michael Resnick. 2 volumes. Foreword by **William A. Wallace.** xlii + xxii + 1,827 pp., apps., bibl., index. Baltimore: Johns Hopkins University Press, 1999. \$116.

These two very handsomely produced volumes appear in the Johns Hopkins series "Foundations of Natural History." They take their place with works of other significant authors in natural history and natural science such as Nicolaus Copernicus, Alexander Von Humboldt, Charles Darwin, and Julian S. Huxley. For too long, the lengthy medieval tomes on natural history have been neglected, partly owing to the lack of translations but also because of early modern judgments concerning their relevance. This work of translation and annotation is first class: the translation is clear and strong, the annotation most helpful. The foreword by William Wallace is a concise and careful summary of the nature of this neglected work.

Kenneth Kittell and Irven Michael Resnick set out to give some sense of the tone and diction of the original. They also wish to convey a sense of the "word choice" and variety of the original, based as it is on Greek, Arabic, and Latin sources. One reason for the importance of the work is the fact that it was composed during the early stages of the formation of the Western scientific vocabulary. Therefore, the translators attempt to maintain the character of the original while making it interesting to the modern reader. Much has been made in postmodern studies about the problem of intertextuality. Kittell and Resnick point to Albertus's *Postilla* style, by means of which he weaves together text and commentary. This is intertextuality *avant la lettre*. As Wallace notes, "In Albert's hands, this technique becomes a powerful hermeneutic device." Yet it gives rise to the issue of what is original and what is the personal contribution of Albertus.

The analytic table of contents is most helpful for the modern reader. Book 1, tract 1, covers the diversity of human parts. Book 1, tract 2, deals with the disposition of the human parts in detail. Book 1, tract 3, deals with the internal parts such as the brain, heart, and lungs. Book 2, tracts 1 and 2, provides a comparison of human and other animals with regard to both internal and external parts. Book 3, tracts 1 and 2, examines the uniform and nonuniform parts of animals, such as the veins, blood, nerves, bone, and hair. Book 4 deals with bloodless marine animals and with sense, voice, and sleep in bloodless animals and the difference between

male and female. Book 5 deals with the generation of animals, with copulation, and with the production of sperm and of the offspring. Book 6 gives an account of the nature, anatomy, and generation of eggs. Book 7 deals with the disposition and life of animals, including the way in which nature moves through gradual intermediaries. It also deals with the health of animals and presents digressions on animal shelter. Book 8 deals with the habits of animals. Book 9 deals with the principles and origins of human generation. In the second tract, one gets an interesting account of both Aristotle and Galen on human generation. Book 10 examines impediments to generation.

The introduction consists of a new account of "the life and works of Albertus Magnus." This has benefited from a careful review of earlier accounts, especially those of the late James A. Weisheipl and Simon Tugwell. Scholars will find these pages helpful. This section is followed by a review of Albertus's importance for medieval science. Once more the contrast between Albertus and the so-called Oxford Platonists like Roger Bacon is emphasized. An account of Albertus's *De animalibus* and its historical context follows.

The translation is rich, with very careful scholarly annotations. The translators are to be praised for this. It enables the modern reader to understand the difficulties of the text.

As a contribution to the study of natural history in the West, this translation will open up new riches to faculty, students, and the general reading public. *On Animals* divides conveniently into three parts. Part 1 consists of books 1–19 and is an exposition of Michael Scotus's translation of Aristotle's *De animalibus* from the Arabic. Part 2 consists of books 20 and 21, Albertus's emendations. Part 3 presents Albertus's *Dictionary of Animals*, taken from Thomas of Cantimpré.

This work will do much to put an end to the simplistic idea that medieval texts were simple repetitions of the Greek originals. In this case, Albertus presents a synthesis of Aristotle and Galen. He knew his Galen both through Avicenna and independently. This phenomenon raises very interesting questions about the nature of Albertus's "Aristotelianism." In brief, then, this work is indeed, as the subtitle states, "A Medieval *Summa Zoologica*."

The glossary, bibliography, and index are most helpful. This translation of Herman Stadler's two-volume Latin edition of Albertus's *De animalibus*, published in the prestigious series "Beiträge zur Geschichte der Philosophie des

Mittelalters" in 1916 and 1920, is a magnificent and remarkable work by two dedicated scholars. It will do much to make medieval natural history available to the world of scholarship and that of the educated reader.

JEREMIAH HACKETT

Monica H. Green. *Women's Healthcare in the Medieval West: Texts and Contexts.* (Variorum Collected Studies Series, CS680.) xx + 388 pp., frontis., tables, apps., indexes. Aldershot, U.K./Burlington, Vt.: Ashgate Publishing Company, 2000. \$111.95.

This collection of Monica Green's essays on women's health care in the Middle Ages represents the finest scholarship being produced today in the history of medieval medicine. The six articles originally published between 1987 and 1998, plus an essay and manuscript handlist appearing for the first time, provide the historian with a treasure trove of information culled from unpublished manuscript sources. This is accompanied by fresh interpretive insights, drawn from a number of different methodological approaches, that place her findings in context.

Green's most important strength is the depth of her codicological research and her commitment to old-fashioned, painstaking philological methods to establish a sound basis for writing history. Her handlist of medieval gynecological texts, found in the appendix to this volume, testifies to her wide reading in European libraries, and the articles in this volume are all grounded in manuscript sources. Two striking examples of Green's textual scholarship are found in the contributions "The *De genecia* Attributed to Constantine the African," which presents a Latin edition of a treatise she identified, Constantine's "On the Genital Members," and the lengthy study "The Development of the *Trotula*," which unravels the immensely complicated history of this group of texts on women's medicine.

Green's careful scholarship is represented throughout the volume in her treatment of both primary and secondary sources. The footnotes often read like bibliographical essays; Green gets hold of every possible article, no matter how obscure or far-flung the publication site, and graciously acknowledges the help of the many scholars with whom she keeps in contact. When using secondary material, she constantly checks the manuscripts on which the studies are based. Green is candid in her assessments of the work of others. She laments, for example, that neither of the two scholars working on different manuscript copies of the same text "made any attempt to com-

pare them systematically with the copy each transcribed" (Essay 1, p. 69); and she presses for more precise definitions of medical practice, referring to the "conceptual carelessness" of some well-respected scholars of earlier generations (Essay 2, p. 341). Green's keen critical sense shows through in every part of this volume.

Another striking feature of these studies is the variety of interpretive tools brought to bear on the questions raised. An important focus of Green's research is to determine who controlled women's health care, and she recognizes the complexities inherent in this investigation. When she comes upon a group of obstetrical and gynecological texts, as we see in the Middle English collection she treats in this volume, Green asserts that "all the texts need to be critically edited, with analysis of the dialect, provenance, and codicological structure of each manuscript. Only then will we be able to assess their full historical import" (Essay 4, p. 56). Green demonstrates how this is to be done in her article on Christine de Pizan. She shows us, for example, how study of the ownership of a book can help us determine if Christine knew "*Trotula*" or how the appearance of a text in surgical codices can confirm that it was a work with medical utility. The newly published article "Women and the Gendering of Medical Literacy" presents a more developed example of this type of analysis, concluding that "the rarity of women's ownership of medical books should not encourage us to dismiss it"; rather, we should "raise our antennas even higher" in our investigation of its limits (Essay 7, p. 48).

The heavy weight of footnotes, variants, and bibliographical citation in this volume in no way compromises Green's skill as a woman's historian. One of her strongest and most successful points is that we must bring analyses developed in many areas of women's history into play in writing the history of women's health care; we need "to raise questions of power and of economic rivalry, of literacy and the control of knowledge" (Essay 1, p. 51). Green does all this and more as she brings together medieval medical history and women's history in a truly masterful way.

HELEN RODNITE LEMAY

Ž. Vesel; H. Beikbaghan; B. Thierry de Crusol Des Epesse (Editors). *La science dans le monde iranien: À l'époque islamique.* (Based on papers presented at l'Université des Sciences Humaines de Strasbourg, 6–8 June 1995.) (Bibliothèque Iranienne, 50.) xxiv + 424 pp., illus.,

bibl., index. Teheran: Institut Français de Recherche en Iran, 1998.

Islamic science is sometimes called Arabic science in order to highlight Arabic's role as the *lingua franca* of science in the premodern Islamic world. However, these expanded conference proceedings on the history of science in Iran illustrate that Persian eventually became a scientific language in Iran (p. xvii). Thus the term "Arabic science" is insufficient. I will direct my comments to the articles in the volume that speak most directly to the extent and scope of Persian scientific texts.

Persian was an important scientific language for about nine hundred years. *Madkhal ilā'ilm aḥkām al-nujūm* [Introduction to Judicial Astrology], which Angelo Piemontese finds to have been composed simultaneously in Arabic and Persian around 975 A.D., is the earliest Persian scientific text cited in the volume (p. 394). At the other end of the chronological spectrum, Elahieh Kheirandish locates a tradition of original

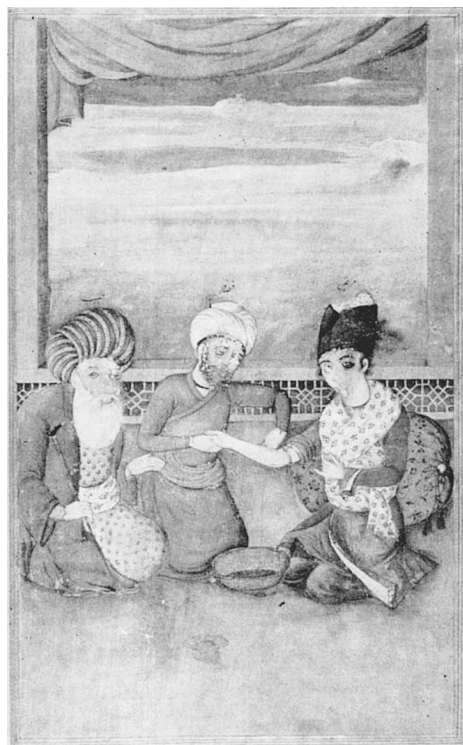
Persian texts on theoretical optics (*manāẓir*) that lasted through the nineteenth century (p. 133). Likewise, Pierre Lory's analysis of the writings of Muẓaffar 'Alī Shāh Kirmānī (d. 1800 A.D.) demonstrates that alchemy remained a component of Shiite philosophy until the same century.

As Persian progressed as a scientific language, an extensive technical vocabulary developed, even in subjects as narrow as ceramics (Y. Porter, pp. 184–186). Other articles, such as those of Mohammad Massoudieh on the musical terms *maqām* and *dastgāh* (p. 363) and Iraj Afshar on *ṣan'at* (p. 155 ff.), concentrate on the evolution of a single term. Parviz Mohebbi shows how such precise Persian technical terminology enabled the reader to construct intricate mechanical devices (p. 192). Clearly, an educated audience consumed these texts.

Lutz Richter-Bernburg's detailed and entertaining account of Kaykāvūs's précis of medicine indicates that Persian stood on its own as a language of learning by the eleventh century. For certain audiences, Arabic was no longer the path to scientific or even religious knowledge (pp. 219, 222). B. Thierry de Crussol Des Epesse explains that a Persian text on eye disease can be read as a source for the social history of twelfth-century Iran (pp. 250–251).

The least flashy but most enduring contribution of this volume is that it brings to light a wealth of untapped textual material in the areas of medicine (A. Naushahi), music (D. Yusupova), astronomy (E. S. Kennedy), physics (Ž. Vesel), and craftsmanship (F. Afkari and H. Taromi-Rad). Additionally, Paul Kunitzsch's identification of an ephemeris of Ulugh Beg's (d. 1449 A.D.) astronomers as an originally Persian text demonstrates that occasionally Arabic scientific texts derived from Persian (p. 41). The volume also features many photographs and diagrams. Several of the full-color plates from Emilie Savage-Smith's account of illustrated Persian medical manuscripts accompany Andrew Newman's essay on Ibn Ilyās's treatise on anatomy.

Increased attention to Persian literature should reveal more fully the link between Islamic science and technology. David King opens the volume with an illustrated discussion of two Iranian world maps, engraved on metal, that present a graphical solution for finding the direction of prayer. (He has subsequently published a book on the subject: David A. King, *World Maps for Finding the Direction and Distance to Mecca* [Al-Furqān Islamic Heritage Foundation/Brill, 1999].) King notes the possibility of a connection between these maps and earlier texts on mathematical geography (pp. 12–13). Jafar



Persian medical illustrations from the Islamic period (from Vesel et al., eds., *La science dans le monde iranien*, p. 312).

Aghayani-Chavoshi's analysis of an early Persian translation of Abū al-Wafā' al-Būzjānī's (d. 998 A.D.) *Treatise on Geometrical Constructions* concludes that scientists and artisans did work together in medieval Iran. A comparison of the Persian with the Arabic original would be instructive, since the two texts do not completely overlap (see A. P. Youschkevitch, *Les mathématiques arabes*, trans. M. Cazenave and K. Jaouiche [Vrin, 1976], p. 108).

Further research on the relationship between Arabic and Persian scientific texts would clarify the real significance of the Persian texts. Piemontese's presumption that Persian texts conveyed the discoveries of the Marāgha astronomers to Copernicus (p. 397) is unsubstantiated. (The Marāgha astronomer whose ideas bear the closest resemblance to those of Copernicus is Ibn al-Shātīr [d. 1375 A.D.], a Damascene who never wrote in Persian.) Rather, do Persian texts extend an earlier Arabic tradition, as Sonja Brentjes suggests in the case of Euclid's *Elements* (p. 77 ff.)? Or, can Persian texts open new vistas, as Berndt Radtke's work on the sources of Islamic cosmography concludes (p. 323)?

ROBERT MORRISON

Roshdi Rashed; Hélène Bellosta. *Ibrāhīm ibn Sinān: Logique et géométrie au Xe siècle.* (Islamic Philosophy, Theology, and Science: Texts and Studies, 42.) xii + 809 pp., figs., glossary, index. Leiden: Brill Academic Publishing, 2000. \$200, €163.

Roshdi Rashed has a vision: to supply the French-speaking scientific community with all the important works of Islamic science. To this purpose, he and his collaborators re-edit texts that have already appeared in a Western language and provide new editions as well. All are accompanied by commentaries as well as carefully edited Arabic texts with the French translations on facing pages.

The present volume contains works by the astronomer and geometer Ibrāhīm ibn Sinān (Baghdad, 909–946), grandson of the famous Thābit ibn Qurra (836–901). Most of Ibn Sinān's extant works have been made available in an Arabic critical edition (A. S. Saidan, *The Works of Ibrāhīm ibn Sinān* [Kuwait, 1983]), but only a few have been translated into a Western language. The texts here presented are an "autobiography by Ibrāhīm ibn Sinān," *On the Method of Analysis and Synthesis in Geometrical Problems*, *On Drawing the Three Conic Sections*, *On Shadow Instruments*, and *Selected Problems* [*Al-masā'il al-mukhtāra*], as well as

Rashed's commentary on Ibn Sinān's quadrature of the parabola reprinted from his *Les mathématiques infinitésimales du IX au XI siècles*, Volume 1 (London, 1996; pp. 678–679, 681–694). Ibn Sinān's two treatises on the subject can be found in the same volume (pp. 696–717, 718–735).

The autobiography is based on the only extant manuscript (Khuda Bakhsh 2519, fols. 1v, 131r–v, 132r–v) and is published here for the first time in a Western language. In this charming little treatise, written after he had turned twenty-five, Ibn Sinān gives a short description of his own work and his attitude toward it.

The second text has been edited and commented on by Hélène Bellosta in her doctoral thesis. Ibn Sinān expected his students first to study his treatise *On Tangent Circles*, unfortunately now lost, as well as Euclid's *Data* and *Elements*, books 1–6. Only then would they be ready to tackle the *Selected Problems*. Bellosta begins with a classification of problems, with geometrical examples, and then moves on to an extensive explanation of how to use analysis and synthesis. As he indicated in his autobiography, by "analysis" and "synthesis" here he means simplified versions, as used by geometers.

About the third text Ibn Sinān says, "As there exists no instrument for drawing conic sections, I have written this treatise in which I have shown how to find as many points as one wants on the conic sections one wants." It contains a careful discussion, with proofs, of how to draw the parabola and ellipse, as well as three methods for drawing the hyperbola. A summary of these point-wise constructions can be found in J. L. Berggren's *Episodes in the Mathematics of Medieval Islam* (New York, 1986; pp. 87–89). Berggren suggests that there were so many constructions of hyperbolas because they were important for instrument makers.

Ibn Sinān was familiar with instruments and their usage. Not only did he write a treatise on movements of the sun (now lost) and perhaps on the astrolabe; he also produced *On Shadow Instruments*, an elementary treatise on sundials, extant in only one incomplete Arabic manuscript (Istanbul, Aya Sofia 4832, fols. 66b–75b). It is important for the history of applied mathematics because of the third problem it discusses: the "hour lines" on a horizontal sundial are not straight lines. Ibn Sinān seems to be the first mathematician who attempted to prove this. Unfortunately, the manuscript breaks off before the end of the proof. P. Luckey's German translation and commentary were published by Jan Hogendijk, who also provided an Arabic critical text—

Luckey's Arabic text having been lost—and an introduction (P. Luckey, "Die Schrift des Ibrāhīm b. Sinān b. Tābit über die Schatteninstrumente" [Phil. diss., Tübingen, 1941], in *Islamic Mathematics and Astronomy*, ed. Jan P. Hogen-dijk [Frankfurt/Main, 1999]).

Selected Problems is Ibn Sinān's most interesting treatise. Here he chose forty-one problems for students who had mastered *On the Method of Analysis and Synthesis*. The treatise is extant in only one disorderly copy (Patna, Khuda Bakhsh 2519, fols. 324, 321, 318, 320, 319, 306–308, 2r–20v), reorganized by Saidan for his edition of Ibn Sinān's works. This was the basis for Bellosta's careful edition and translation. In this text Ibn Sinān refers to and criticizes several colleagues and even uses a theorem from his grandfather's *Book of Assumptions* (see Y. Dold-Samplonius, "The *Book of Assumptions* by Thābit ibn Qurra [836–901]," in *History of Mathematics: States of the Art*, ed. J. W. Dauben et al. [San Diego, 1996], pp. 207–222). As Rashed and Bellosta point out, Sinān's *Selected Problems* depends heavily on works by Apollonius, works now lost except for *Cutting Off a Ratio* and mostly known through quotations by Pappos. The relations between Ibn Sinān and Apollonius have been expounded by Jan Hogen-dijk ("Arabic Traces of Lost Works of Apollonius," *Archive for History of Exact Sciences*, 1986, 35:187–253). This publication was apparently not known to Rashed and Bellosta.

Ibrāhīm ibn Sinān was a great scientist. The careful translations, published here for the first time, fill a gap in our knowledge about him. They are of real importance—and not only for the French speaking. We should be grateful to the authors for this volume.

YVONNE DOLD-SAMPLONIUS

Jürgen Helm; Annette Winkelmann. *Religious Confessions and the Sciences in the Sixteenth Century*. (Studies in European Judaism, 1.) xiv + 161 pp., index. Boston: Brill Academic Publishing, 2001. \$54 (cloth).

The title is the most interesting part of this collection of papers, the result of a conference that took place in Wittenberg in 1998 at the Leopold Zunz Centre for the Study of European Judaism. The conference was designed to address two questions: "(1) To what extent was scientific thought influenced by religious traditions and beliefs, and (2) did the achievements of sixteenth-century natural sciences and medicine have an effect on religious ideas?" (p. xi). The papers, by and large, are disappointing. With

very few exceptions, they are based on outmoded historiographical assumptions and, consequently, fail to provide interesting answers to these important questions.

This short volume is divided into three sections: "Christian Confessions and the Sciences," "Ways of Transmission," and "Judaism between Tradition and Scientific Discoveries." The first section is the strongest. In "Melancthon and the Tradition of Neoplatonism," Günther Frank revises the traditional interpretation of Melancthon as an Aristotelian by arguing that his natural philosophy owed a considerable debt to Plato's *Timaeus*. Unfortunately, Frank's paper is less innovative than he proclaims, because Platonic themes have already received some attention in Sachiko Kusukawa's *The Transformation of Natural Philosophy: The Case of Philip Melancthon* (Cambridge, 1995), an important book that Frank never mentions. Jürgen Helm's paper, "Anatomical Education at Wittenberg and Ingolstadt," is perhaps the strongest in the volume. Helm compares anatomical teaching at these universities in order to examine possible differences between Lutheran and Catholic traditions. After a thorough examination of substantive and curricular issues, Helm concludes that the teachers at both institutions were willing to adopt the new results of (largely Vesalian) sixteenth-century anatomy but that anatomy played different roles in the curricula of these universities. At Ingolstadt, the Catholic university, the study of anatomy was confined to the medical curriculum, while at Lutheran Wittenberg anatomy was studied as an integral part of natural philosophy and was taught within a theological framework as an empirical example of God's handiwork.

The latter two sections of the book are less satisfying. Instead of trying to understand the contexts and assumptions of medieval and early modern Jewish thought, the contributors seek either evidence of "modern" ideas within the writings of various Jewish thinkers or lines of influence leading to modern science. The real problem posed by looking for connections between religion and science in early Jewish thought is that, with a few notable exceptions, the central European Jewish community tended to reject secular learning and was largely out of touch with the developments of the day. The exceptions tended to be located in the more open communities in Italy, Provence, and Spain. David Ruderman covered much of this ground in *Jewish Thought and Scientific Discovery in Early Modern Europe* (Yale, 1995). Bernard Goldstein, Tzvi Langermann, and Menachem Fisch have written extensively on serious sci-

entific work within the Jewish community, but the authors of the present volume have not paid sufficient attention to their scholarship.

In addition to suffering from these conceptual problems, the volume lacks good editing. Some of the authors have a rather unsteady grip on English, the text is full of strange neologisms such as “Vesal” and “Hippocratism,” and there are other similar problems that a good copy editor ought to have removed or corrected.

MARGARET J. OSLER

Giordano Bruno. *Oeuvres complètes*. Volume 7: *Des fureurs héroïques*. Translated by Paul-Henri Michel. Introduction by Miguel Angel Granada. (Bibliothèque Italienne sous le Patronage de l'Institut Italiano per gli Studi Filosofici.) cxi + 625 pp., indexes. Paris: Les Belles Lettres, 1999. (Paper.)

This last volume of Giordano Bruno's Italian dialogues is the final one that he wrote during his London sojourn (1583–1585). The fact that each of these works has now been translated into French makes them readable for a more general audience on the European Continent and perhaps in the United States as well. I do not believe, however, that the translation of Bruno's Italian into French is the only benefit of this enterprise, although the translations are good and useful. The greater benefit lies in the volumes' interpretive introductions and in their rich footnotes.

This is especially the case with the publication of *Des fureurs héroïques*. Miguel Angel Granada's introduction revisits Bruno's three metaphysical dialogues (*La cena de le ceneri*; *De la causa, principio et uno*; *De l'infinito, universo et mondi*), his three moral dialogues (*La cabala del cavallo Pegaseo*; *Lo spaccio della bestia trionfante*; *De gl'heroici furori*), and his comedy (*Il candelaio*). Granada makes interesting arguments concerning not only the meaning of *Furori* but also about how all these works fit together and how arguments even in the first (*Cena*) lead to the poetic narrative of this last work. Granada argues that Bruno arrived in England with a goal in mind about his total message to Albion. Although Granada does not say so, to some degree his view confirms Frances Yates's idea that Bruno had a “mission” to the English. Whether it had anything to do with the goals of Henri III, as Bruno said, or only with Bruno's fertile mind, we will probably never know.

De gl'heroici furori clearly shows that Bruno had replaced the Christian representation of man's union with God in the next world with a communion of man with divine, infinite nature

through philosophical contemplation. This conception is tied to the destruction of the finite and hierarchized Aristotelian cosmos that began in the *Cena de le ceneri*. Indeed, Granada argues that “the idea of the *Spaccio* and *Furori* was clear and precise in Bruno's mind from the beginning [of his Italian dialogues]” (p. xlii). Furthermore, Bruno's use of the conception of his own gospel in *De l'infinito* (and, I might add, in the poem “Al malcontento” at the beginning of the *Cena*) is an alternative to the Christian gospel and mystery (p. xlviii).

Furori discusses the *furioso* (Bruno himself, to be sure, and probably his disciples, who are true Acteons on the hunt for the true Diana). “Very rare,” Bruno says, “are the Acteons who can contemplate the naked Diana.” For Bruno, communion with the divine occurs through philosophical contemplation, which is made possible by infinite nature but only to these “rare men” (a concept found for the first time in the *Cena*). As Bruno will later say in *De immenso*, “that is why [the perfect philosophical] man will be called by Trismegistus a great miracle, he who is able to transform himself into God as if he himself was God” (*Op. Lat.* I, 1, p. 205). There are, he adds, false Acteons and false Dianas (priests and Christian believers who follow the Pope and Christ rather than nature). Again, Granada points out, the theme of true Acteons/philosophers versus false Acteons and Dianas can be found in the *Cena* and the *Spaccio*; there they are part of the civic religion whose purpose is only to keep the masses within the bounds of moral behavior.

To Bruno, the intellectual hunt of divinity leads to Wisdom (*sapientia*) and a state of perfection that, thanks to “speculative sciences,” ultimately leads to *copulatio* with divinity, which is the metamorphosis of Acteon. He has attained his divine object and “feeds himself on Truth, in a state that is no longer ‘furioso.’” He lives the life of God, “feeding and drinking on ambrosia and nectar.” Bruno refers to Luke 17:21 and 2 Corinthians 4:16: “the kingdom of God is within us” and “the interior man renews himself from day to day.” In sum, the only possible union for “perfect man” with God is through infinite, intelligible space (p. lxxxviii) and not in the Christian Heaven.

Upon being condemned by the Roman Inquisition on 8 February 1600, Bruno said, “You who condemn me are more fearful than I am who has been condemned.” He believed that theirs was a Christian fear, while he, the true philosopher, did not fear death. At the end, Bruno did not have to make amends with the Christian religion, for he believed his soul was already one

with God. His Italian dialogues had already showed true English “Acteons” this path to the true Diana as well.

The editors and translators of *Furori* and of the entire Italian dialogues are to be congratulated on the quality of their critical editions as well as for their readings of the texts themselves. We can look forward to the continuing critical editions of the Latin works of Giordano Bruno.

EDWARD A. GOSSELIN

William T. Lynch. *Solomon's Child: Method in the Early Royal Society of London.* xi + 292 pp., bibl., index. Stanford, Calif.: Stanford University Press, 2001. \$60 (cloth).

By “Solomon,” William T. Lynch means Francis Bacon; by his “child” (more properly “children”), Baconian methods, programs, and institutions; and by “early,” the first quinquennium of the official existence of the Royal Society. Through useful, insightful, and richly documented exegeses of John Evelyn’s *Sylva; or, A Discourse of Forest Trees* (1664), Robert Hooke’s *Micrographia* (1665), Thomas Sprat’s *History of the Royal Society* (1667), John Wilkin’s *Essay toward a Real Character, and a Philosophical Language* (1668), and John Graunt’s *Natural and Political Observations . . . upon the Bills of Mortality* (1662), Lynch demonstrates connections between the first books licensed by the society and its Baconian heritage. So? The society itself claimed the connection; the *Encyclopédie* trumpeted it to the world; and the Baconianism of English gentlemen of the Restoration is a frequent theme in the recent historiography of science. Much of Lynch’s effort goes to pushing against an open door.

He pushes further than others, however, in offering to correct historians who believe that the Royal Society began to do real science only when it distanced itself from Baconian methods. No doubt the early Royal Society did “science”—that is, enriched and discussed what it called “natural knowledge”—under Bacon’s flag of fact-finding before theory-making; and Lynch properly calls attention to the wide range of productive inquiries that thus fell within the society’s purview. But to make good his case that the investigations he reports followed Baconian methods or mandates closely, he interprets Solomon so loosely that all natural philosophers become his children.

Hooke’s discussion of the cause of capillary action may serve as an *experimentum crucis* for Lynch’s principles of exegesis. Hooke regarded the mechanical philosophy—the house philos-

ophy of the Royal Society and one Bacon himself had employed—as unproblematic. But in accounting for capillary rise, Hooke had recourse to the concepts of congruity and incongruity. What causes congruity? The mechanical philosophy required the answer: the sizes, shapes, and motions of the particles of air, water, glass, and, perhaps, a subtle ether. Lynch argues that here Hooke merely built out the mechanical philosophy in a way consonant with Bacon’s allowance of hypothesis. But were not congruence and its opposite occult qualities, throwbacks to the scholastic nonsense against which Bacon had directed his loudest artillery? And were they not therefore unallowable in sound philosophy? Lynch: On the contrary; Hooke promised to explain how they worked mechanically; we should regard them not as violations of, but as extensions to, the mechanical philosophy. This escape is not available. The unknown cause, mechanical or not, of a manifest effect was precisely what the schoolmen meant by an occult quality. Hooke fell off the edge of the Baconian world with his congruities. Indeed, Lynch grants as much in arguing that they prefigured Newton’s sociabilities and universal gravitational attraction.

The first issues of the *Philosophical Transactions* may be taken as one indicator of the interests of the early Royal Society. Probably the best-represented science in these issues was astronomy. Lynch does not concern himself with it. He does not discuss the methods of fellows interested in mathematical topics and secures his main thesis by interpreting Bacon’s directives with what strict constructionists ought to regard as undue laxity. It appears that Lynch’s method is insufficiently Baconian. His fine case studies are too few in number and too narrow in variety to capture the methods at work in the early Royal Society.

J. L. HEILBRON

■ Early Modern (Seventeenth and Eighteenth Centuries)

Michael Oberhausen; Riccardo Pozzo (Editors). *Vorlesungsverzeichnisse der Universität Königsberg (1720–1804).* (Forschungen und Materialien zur Universitätsgeschichte, 1.) 2 volumes. lxviii + 778 pp., illus., indexes. Stuttgart: Frommann-Holzboog, 1999.

These volumes reprint the semesterly *Catalogus praelectionum* (in Latin), or “catalogue of lectures,” at the Academy of Königsberg, which came to be known as the University of Königs-

berg. The period covers the lifetime of Immanuel Kant (1724–1804), who attended the university from 1740 to 1748, then became a *Privatdozent* in 1755 and Professor of Logic and Metaphysics in March 1770. The first volume includes the lecture lists prior to the latter date, the second those up to the winter semester 1803/1804, when Kant died (having ceased lecturing some eight years earlier). Coincidentally, the format of the catalogues changed with the winter semester 1770/1771. Previously, the lectures were listed under the name of each professor, starting with the semester's rector and followed by the regular professors (so not including the exclusively private lecturers, or *Privatdozenten*) ordered hierarchically by faculty—theology, law, medicine, and philosophy—with the topics of their public and private lectures. Subsequently, the lectures were listed by topic, now including those by *Privatdozenten*, and were marked (and later listed separately) as public and private. (Many of Kant's lectures were private, even after he became a professor.) The philosophy lectures were now listed under four subheadings: philological (rhetoric, literature, and classical languages, including Greek, Latin, Hebrew, and Arabic), historical, mathematical, and philosophical. Instruction in modern European languages was listed separately and included French most often but also English, Italian, and Polish. "Bodily" disciplines came last (horse riding, use of arms, dancing, painting, music). From 1771 to 1794 there was nearly always a brief prefatory note, presumably by the rector, though the apparatus does not attest to the authorship.

The fifty-page introduction (in German) describes the significance of the lists, characterizes changes and developments in the philosophical faculty and philosophical instruction, gives an overview of the required curriculum at Königsberg from 1770 (for all four faculties), describes the use of Latin and its decline at century's end, notes that the main source for the printed catalogues was the collection assembled by the Kant scholar Rudolf Reicke (1825–1906), and annotates the indexes, which cover (a) Bible citations, (b) instructors, and (c) other named persons, authors, works, and handbooks. Beyond its interest for Kant studies, the editors offer the collection as providing insight into German university instruction in the Enlightenment, subject to the disclaimer that not all the lectures listed were actually given and not all the works used as a basis for lecturing were treated favorably. Of special note is the growth of the philosophy faculty, which marked the development of the "arts and sciences" from being merely preparatory to the

higher faculties of law, medicine, and theology into a higher faculty in its own right. The list of authors and works offers a bibliographical overview for the areas of instruction. For the historian of science, the lecture lists permit tracking of the teaching of physics, mathematics and mathematical sciences, chemistry, natural history, psychology, anthropology, and statistics. "Statistics" (*statistica*) was taught using works by Gottfried Achenwall (1719–1772) that offered descriptions of "noteworthy" facts about the various states of Europe (including political and legal systems, political geography, and various economic and military facts) to permit comparisons among states and the tracking of changes in individual states over time. Readers generally might have wanted more information about the structure of the catalogues and their production. A comprehensive list of lecture topics would have been useful, for they are not all covered by the detailed overview of the curriculum. The volumes are nicely produced in large format and should be welcomed as useful tools for studying university education in eighteenth-century Germany.

GARY HATFIELD

Douglas M. Jesseph. *Squaring the Circle: The War between Hobbes and Wallis.* (Science and Its Conceptual Foundations.) xiv + 419 pp., figs., app., bibl., index. Chicago/London: University of Chicago Press, 1999. \$80, £56 (cloth); \$28, £20 (paper).

The main subject of Douglas Jesseph's book is Thomas Hobbes's philosophy of mathematics, analyzed lucidly and comprehensively in its three central chapters. These focus, respectively, on the material in *De corpore* related to mathematics; on Hobbes's exchanges with John Wallis about such notions as magnitudes, ratios, the angle of contact, and the infinite; and on Hobbes's criticism of analytic geometry. Another chapter analyzes in some detail Hobbes's mathematical tracts of his last years, including the disputes around his alleged duplication of the cube. Two more chapters deal with the political and religious differences pitting Hobbes against Wallis from the 1650s through the end of Hobbes's life. They are informative and summarize competently the vast literature on the subject. Perhaps the only criticism that can be addressed to this otherwise excellent book is that there is no real symmetry between the actors in it. In the final analysis, Jesseph's Hobbes is an idiosyncratic old man, driven by arrogance and too much faith in his peculiar philosophy of mathematics, while

Jesseph's Wallis is a sound, modern-minded mathematician, if perhaps a little too contentious. Not inconsistently, Jesseph's exhaustive analysis of Hobbes's philosophy of mathematics is not matched by an equivalent exploration of Wallis's.

Jesseph's clear, careful account of the major issues in Hobbes's philosophy of mathematics is generally well informed and persuasive enough. Yet I cannot agree with his understanding of a particular but fundamental point. When Jesseph discusses the relations between Hobbes's mathematics and "physics," he takes at face value Wallis's criticism that Hobbes's philosophy of mathematics rests on "physical" concepts (pp. 132–135). Many pages later (p. 223), in a different context, Jesseph acknowledges a tension between Hobbes's allegedly physical foundations of mathematics (i.e., notions such as body, space, time) and his claims about the conjectural nature of physics and then recognizes that to Hobbes notions such as body, space, and time are "the first principles of metaphysics." All of this suggests that Hobbes wanted his mathematics grounded on metaphysics rather than on physics, and yet the reader is left with the definite impression that Wallis was right and that Hobbes's materialistic philosophy of mathematics "places physical concepts" at its "center." Be that as it may, the book contains a cogent, fair, and overall sympathetic account of Hobbes's philosophy of mathematics that is complemented with a useful presentation of the many authors and influences shaping it. Jesseph does an excellent job of delineating the ties between the philosophical disputes on which the book focuses and the political and religious differences that separated Hobbes and Wallis.

Finally, it must be noted that Jesseph provides a detailed analysis of Hobbes's views on algebra, which are placed against a competent summary of his views on language and demonstration. Hobbes's arguments against algebra and his criticism of Wallis's use of algebraic methods provide important new evidence on the debate about the roles of algebra and geometry in seventeenth-century mathematics, a debate that is far from being settled. Jesseph's clearly and elegantly written book is a welcome and valuable contribution to Hobbes studies and to the literature on seventeenth-century philosophy of mathematics.

ANTONI MALET

Ladina Bezzola Lambert. *Imagining the Unimaginable: The Poetics of Early Modern As-*

tronomy. 182 pp., illus., bibl. Amsterdam: Rodopi, 2002. \$34.50 (paper).

The title and subtitle of this monograph are seriously misleading. A book on imagining the unimaginable should concern itself with the heart of this oxymoron: though the imagination has common sensation as its only basis, it nevertheless produces something unique—namely, new knowledge. This is not Ladina Lambert's topic, however; she is concerned, as she herself says, only with the capacity of the imagination to reorder the mind's visual contents: to create satyrs out of images of men and goats or, more to the point, craters out of moon shadows. Nor is this really a book about early modern astronomy. True, it concerns itself with Galileo, Kepler, and Huygens. Galileo's *Sidereus Nuncius* and his *Dialogue Concerning the Two Chief World Systems*, works certainly central to early modern astronomy, are the foci of a chapter. But the choices in the cases of Kepler and Huygens are eccentric indeed: Kepler's *Somnium* and Huygens's *Kosmotheoros*. It is equally odd that a book professedly about early modern astronomy should deal so extensively with Ariosto's *Orlando Furioso* and two novels each of Cyrano de Bergerac and Italo Calvino. Readers might initially expect that there is at least some unity of theme, since the first four chapters concern themselves with envisioning the moon. But even these expectations are destined to be defeated: the fifth chapter is about the plurality of worlds and the sixth and last about representing the unimaginability of the imaginable in the novelist Italo Calvino. Astronomy makes no appearance.

But all of this carping is beside the point: *Imagining the Unimaginable* is really a work of literary criticism, one that reclassifies science as literature. Although the author occasionally uses the term "rhetoric," this is not a book about rhetoric in the traditional sense, the sense in which it concerns making a persuasive case. *Imagining the Unimaginable* is a book about creating plausible worlds with words, in the tradition of Aristotle's *Poetics*. At the center of this process is analogy, not—as Lambert asserts—metaphor. When I compare genetic processes to a code, I am employing metaphor; I am comparing two things that are, initially at least, radically *unlike*. But Kepler, for example, created his moon inhabitants, the Privolvans, out of their likeness to human beings, telling us only what human beings might have become under radically differing conditions. The imagination, according to Lambert's definition, merely reorders; it does not recreate.

The author's insistence on metaphor over analogy is a symptom of a general strategy: the reclassification of scientific works as literary. Galileo and Ariosto, Kepler and Italo Calvino, Huygens and Cyrano de Bergerac—it certainly says something about the scientists in each of these pairings that, at least some of the time, they functioned in ways parallel to writers of fiction. Nonetheless, the desire to reclassify works of science as works of literature seems misplaced if the goal is insight into science. While there are exceptions—*Uncle Tom's Cabin*, for example—works of literature seem to be largely ends in themselves. Works of science, however, are instrumental. In *Sidereus Nuncius*, Galileo wanted to convince his audience that the moon had craters; in the first *Dialogue*, he wanted to convince them that heliocentricity was true. Even in the case of Kepler in his *Somnium* or Huygens in his *Kosmotheoros*, although conviction cannot have been the goal, one assumes that responsible speculation was. Though intelligent and perceptive, *Imagining the Unimaginable* will leave most of those interested in science bemused. For those not bemused, the lack of an index is bound to be infuriating, as it unreasonably raises the cost of using this book for any scholarly purpose.

ALAN GROSS

Allen G. Debus. *Chemistry and Medical Debate: Van Helmont to Boerhaave*. 296 pp., illus., bibl., index. Nantucket, Mass.: Science History Publications, 2001. \$52 (cloth).

Allen Debus has spent the greater part of an ongoing intellectual life constructing a space for chemistry and medicine within the house of the early modern "scientific revolution." In earlier works he has described the debates between iatrochemists on the one hand and Aristotelians and Galenists on the other and has illustrated the role of a new view of nature—one in which the world could be understood through chemistry and chemical analogies—in establishing the new learning of the seventeenth century. The present volume extends the story into the period in which the rise of the mechanical philosophy is generally viewed as putting an end to debates about "chemical philosophy." As Debus shows, in the late seventeenth and early eighteenth centuries mechanist physicians, those who sought to apply the methods of mathematics and physics to medicine, found themselves quarreling, albeit with different arguments, with the same antagonists that had confronted Aristotelians and Galenists before them—chemical physicians.

Those familiar with Debus's other books will find that the earlier chapters of *Chemistry and Medical Debate*, especially, repeat much of what has been written in earlier monographs. Initial chapters depict the intellectual concussion caused in the medical world by the work of Paracelsus and Jean Baptiste van Helmont and then present examples of the textual response to the publication of van Helmont's *Ortus medicinae* in two works, both published in 1657, written by Gabriel Fontaine and Franciscus Oswaldus Grembs. There follows a description of the medico-chemical views of Franciscus de la Boë Sylvius and Thomas Willis, the latter especially important for clearly joining together anatomy and chemistry and in arguing that ferments and fermentation were the basis for comprehending the operation of the human body and the macrocosm. Here too Debus notes Robert Boyle's early infatuation with van Helmont and presents the debates that surrounded the Helmontian chemical physician Marchamont Nedham in regard to the nature of the relationship between chemistry and medicine and the founding of the Society of Chemical Physicians in 1665. The textual history of arguments encircling the acid/alkali theory and attention to Otto Tachenius, who ascribed the origins of the theory to Hippocrates, leads to a description of the iatrophysical works of Giorgio Baglivi, Archibald Pitcairne, and Philippe Hecquet, each of whom, contending with the arguments of prominent iatrochemists like Raymond Vieussens and Jean Astruc, envisioned a place for a mathematical-mechanical approach within medicine and presented mechanistic interpretations of digestion.

Moving into the early eighteenth century, however, Debus describes the positions of two contemporaries, Hermann Boerhaave and Georg Ernst Stahl, whose views had the greatest effect on the ongoing debate between chemists and mechanists. Debus describes Boerhaave as a mechanist who attacked ancient authors (with the exception of Hippocrates) and rejected chemistry as the basis for understanding physiology and the processes of life. He therefore sought to separate chemistry and medicine. Stahl too viewed chemistry as incapable of explaining life but rejected the positions of atomists and mechanists, preferring to argue for the presence of a vitalistic "anima" as that which orchestrates life processes. Whether mechanist or animist, however, both thought that chemistry, while still a servant to medicine, was a field quite different from medicine; and it is in this regard that their work represents, according to Debus, a "funda-

mental break” with the Paracelsian-Helmontian tradition.

Allen Debus has produced a wonderful survey of chemical-medical debate in the early modern era and has opened up avenues for further inquiry by future historians. The only quibble here is with a perhaps too strict categorization of intellectual points of view and opinions. Rather than ignoring him, for instance, Boerhaave lists Stahl at the beginning of his *Elements of Chemistry* among the authors who had contributed most to medicine and natural philosophy. Also, treating Friedrich Hoffmann solely as a mechanist seems problematic in light of other interpretations. Of a more basic nature is a problem with the book’s presentation. Science History Publications did not serve this volume well when it neglected to print whatever text ought to have been found on page 30.

BRUCE T. MORAN

Stillman Drake. *Essays on Galileo and the History and Philosophy of Science.* Volumes 1–3. Edited with introductions by **N. M. Swerdlow** and **T. H. Levere**. Volume 1: xxiv + 473 pp., frontis., illus., index; Volume 2: viii + 380 pp., frontis., illus., figs., tables, index; Volume 3: vi + 392 pp., frontis., illus., figs., tables, bibl., index. Toronto: University of Toronto Press, 1999. \$75 (cloth); \$24.95 (paper).

Scholars of the seventeenth century, and especially Galileo groupies, owe a great debt to Noel Swerdlow and Trevor Levere. These two have done an excellent job of bringing out three volumes of Stillman Drake’s collected papers. Since many of these essays were originally published in anthologies or in journals not easily accessed, having them available in one place (or three, actually) is a real boon.

The essays here span thirty-six years. Reproduced in these three volumes are some gems that may have escaped notice when first published. Volume 1 deals with Galileo: biography, textual studies, philosophy of science, and astronomy; Volume 2 presents essays concerning the *Dialogue* and *Discourses* (including the inclined plane papers); Volume 3 treats instruments, general seventeenth-century studies, and some quite different papers on A. B. Johnson and philosophy of science in general.

I had forgotten how insightful and detailed in analysis were Drake’s papers on Galileo’s and, more generally, proportional mathematics, and how fascinating were the essays on music. I was sorry not to find a favorite of mine, “Galileo and the Telescope,” where Drake’s detective work

pieces together Galileo’s steps through Padua and Venice as he prepares to construct his first instrument. (There was an early version of this in *Isis*, 1959, 50:245–254, then a revised version in Stillman Drake, *Galileo Studies* [Michigan, 1970].) But editors must make choices, and Swerdlow and Levere have chosen well.

Stillman Drake was the “dean” of Galileo scholars. He spent his life working through the texts and manuscripts. Even those who disagreed with Drake’s interpretation of Galileo’s work knew him as the authority against which they had to react. It is of great value and a pleasure to have his essays all together (or 80 percent of them). In reading and rereading these essays, one can only admire the erudition and knowledge that comes from a lifetime of dedicated and creative scholarship.

PETER MACHAMER

J. Christiaan Boudri. *What Was Mechanical about Mechanics: The Concept of Force between Metaphysics and Mechanics from Newton to Lagrange.* Translated by **Sen McGlinn**. xvi + 276 pp., figs., bibl., index. Dordrecht: Kluwer Academic Publishers, 2002. \$112 (cloth).

Two main points must be made about this book. First, it successfully focuses on a significant but underdeveloped area in the history of modern physical science, the role of metaphysics in the evolution of mechanics. Second, a surprising deficiency in scholarship seriously impedes the development of a major topic in the study, the role of force in Leibniz’s dynamics.

J. Christiaan Boudri’s investigation has two main goals: first, “to defend the general proposition that the historiography of science should take account of metaphysical suppositions”; second, “to unravel the broad lines of development in metaphysics itself and in its relationship to mechanics” (p. 4). Both goals are accomplished, the first perhaps better than the second.

The main argument is that the evolution of modern mechanics reflects the evolution of the concept of force and, most important, that force, from the beginning, is a metaphysical concept, one whose content is tightly linked to the notion of substance. Thus, right from the start, with Leibniz and Newton, “it is clear how much the mathematization of the concept of force in the eighteenth century took the substantial concept of force as its starting point” (p. 99). Following these two founders, the focus shifts to D’Alembert’s “purification of metaphysics” (Ch. 4) and then to the foundation of the principle of least

action—with its troublesome connection to final causation—in Maupertuis and Euler (Ch. 5).

Chapter 6 examines the role of the philosophical competitions sponsored by the Berlin Academy, most particularly that of the essay contest of 1779, whose set question on the nature of force marked the culmination of the explicit (but most certainly not the implicit) role of force in mechanical thought. Boudri's discussion here is clear and careful, providing the richest extant analysis of this fascinating enterprise. In Chapter 7 Lagrange's "post-metaphysical" mechanics is analyzed, with special reference to the now fully mathematicized principle of least action.

Boudri's conclusion appears in Chapter 8. His strongest, most specific point, one well worth the making, is that in order to understand developments in eighteenth-century mechanics, it is not enough to take developments in metaphysics into account: these mechanical developments must themselves be seen as developments in metaphysics. Boudri also has a more general, historiographical point: "making the metaphysical premises of a scientific discipline visible . . . provides a better insight into its historical development" (p. 231). This conclusion is forgotten by historians of science at their peril.

The book exhibits a curious deficiency in scholarship, most evident to me in the extended and seminal analysis of Leibniz's development of the force concept. Boudri admits his debt to the three classic scholars of eighteenth-century science—E. J. Dijksterhuis, I. B. Cohen, and Richard S. Westfall—which is fair enough. Yet when he claims that other scholars in the decades since the work of these three in the early 1970s "have not come significantly farther . . . in relation to the interpretation of the relationship between force and substance" (p. 35), he is most assuredly mistaken. Throughout the 1970s a lively international debate took place regarding the role of force in Leibniz's dynamics. This debate, frequently using *Studia Leibnitiana* for its venue, climaxed with the 1982 international symposium on Leibniz's dynamics at Loccum, a meeting whose proceedings were duly published as a special volume by Hannover's Leibniz Gesellschaft. Boudri's bibliography contains no references to this long debate.

Because of this surprising lapse in scholarship, Boudri's discussion of Leibniz on force—the skeleton of the book—exhibits not just a reinventing of the wheel but serious incidences of labored thought that could have been much eased by assistance from those who had already been there and done that.

Still, even with this flaw, *What Was Mechan-*

ical about Mechanics is a valuable resource for all those interested in the history of mechanics.

GEORGE GALE

Matthew R. Edwards (Editor). *Pushing Gravity: New Perspectives on Le Sage's Theory of Gravitation*. iv + 316 pp., figs. Montreal: Apeiron Publishers, 2002. \$25 (paper).

The Genevan physicist Georges-Louis Le Sage (1724–1803) is known for his attempt to provide a mechanical explanation of gravity. His basic assumption was the existence of so-called ultramondane corpuscles of minute mass and high speed that strike bodies from all sides. From these premises he deduced an attractive force between two macroscopic bodies, arguing that each of these bodies would block some of the impinging particles, with the result that the bodies would be impelled together. According to this theory, attraction between gross objects is an illusion; gravity is the result of pushing particles. Le Sage claimed that this impulsive force would vary inversely as the square of the distance between the bodies, in accordance with the Newtonian law.

Most of the twenty-three essays in this collection deal with rather speculative modern Le Sage-type theories. In some of them, Le Sage's ultramondane particles are replaced by quanta of gravity ("gravitons") or by photons. Their authors introduce quantized properties of astronomical objects ("an ignored and therefore suppressed subject" [p. 5]), particles traveling much faster than the speed of light ("yet another strike against the hallowed relativity theory" [p. 5]), and other objections against the theory of relativity, described as the "dogma of curved space time" (p. 6). Martin Kokus's concern is "further questioning of scientific dogma" (p. 297), and in his essay "Alternate Theories of Gravity and Geology" we learn that "continental drift is just an illusion" (p. 294). Some of the "new perspectives" are not as new as the title suggests. The Romanian physicist Nedelia Popescu-Adamut summarizes an "electro-thermodynamical theory of gravitation" elaborated by her late father around 1970; a contribution by two Russian physicists was originally published in 1960. I frankly admit my incompetence to do justice to such alternatives to established physics.

Only five essays are of interest for historians of science. James Evans ("Gravity in the Century of Light: Sources, Construction, and Reception of Le Sage's Theory of Gravitation"; pp. 9–40) offers a well-researched presentation of Le Sage's life and scientific career, the genesis of

his model, and its reception by contemporary scientists. Frans van Lunteren's contribution ("Nicolas Fatio de Duiller on the Mechanical Cause of Universal Gravitation"; pp. 41–59) deals with a Genevan forerunner of Le Sage who presented a particle-based theory of gravity to the Royal Society in 1690. Newton's own views on gravitation are discussed in a short paper by the late E. J. Aiton ("Newton's Aether-Stream Hypothesis and the Inverse Square Law of Gravitation"; pp. 61–64), a reprinted extract from an article originally published in 1969. Matthew R. Edwards ("Le Sage's Theory of Gravity: The Revival by Kelvin and Some Later Developments"; pp. 65–78) discusses the brief revival of Le Sage's model in the late nineteenth century and the debates on this subject among Kelvin, Maxwell, and other classical physicists. Roberto da Andrade Martins ("Majorana's Experiments on Gravitational Absorption"; pp. 219–238) presents the work of Quirino Majorana, who, around 1920, claimed to have measured a "gravitational absorption" effect similar to the gravitational shielding predicted by Le Sage.

The references given in the various papers adhere to very different—more and less scholarly—standards. V. V. Radzievskii and I. I. Kagalnikova claim in "The Nature of Gravitation" (pp. 79–91) that the theory discussed in this book stems not from Le Sage but from the Russian scientist M. V. Lomonosov; contrary to all the other contributors, they consistently call it "the theory of Lomonosov and Le Sage" or "the Lomonosov–Le Sage hypothesis." The only reference given for this important assertion is Volume 1 of Lomonosov's collected works—no page, no paper title, no year of publication. Another flaw of the book is the absence of an index of names.

ANDREAS KLEINERT

Renate Wilson. *Pious Traders in Medicine: A German Pharmaceutical Network in Eighteenth-Century North America.* xiv + 258 pp., illus., figs., bibl., index. University Park: Pennsylvania State University Press, 2000. \$37.50.

Renate Wilson has discovered the strong influence of the trade in proprietary medicines from Protestant Germany during the eighteenth century on the spiritual, organizational, medical, scientific, and economic development of Pietist communities in America. This early transatlantic commerce originated from the Halle Orphanage—"Waisenhaus"—Foundation and its large pharmaceutical manufactory. After 1696 Halle became an important Prussian university town,

the center of a philanthropic and religious foundation during an epoch when Pietism was predominant and mercantilism sought to promote economic prosperity. The title of Wilson's book is misleading insofar as she deals only with the significance and role of the Halle Orphanage Foundation and its worldwide trade in Bibles and, above all, medicaments. Everywhere, the Halle medicaments had the aura of piety: the foundation was organized with the help of pious persons' legacies and bequests; Pietist physicians and apothecaries worked in its hospital, pharmacy, and pharmaceutical manufactory; and Pietist ministers distributed the medicaments.

Today the Francke Orphanage Foundation at Halle maintains a "*copious and well-preserved*" (p. x) archive that, before the political changes in Germany in 1989 and 1990, was minimally accessible; only the Halle medical historian Wolfram Kaiser and his collaborators used it freely, and we are indebted to them for most of the important historical publications on the institution (see Arina Völker, "Verzeichnis der medizinhistorischen Veröffentlichungen von Wolfram Kaiser," in *Dixhuitième: Zur Geschichte von Medizin und Naturwissenschaften im 18. Jahrhundert*, ed. Völker [Martin-Luther-Universität, 1988], pp. 214–242). Scholars will be grateful that Wilson's intensive and meticulous studies of the archival material have widened our knowledge not only of the foundation's religious history but also its economic history and—above all—its place in the history of science, medicine, and pharmacy.

The first three chapters offer thorough new accounts of "The Francke Orphanage Foundations at Halle: The Fruit of Religious Reform," "Pietist Medicine and Medical Education: A Radical Heritage Tempered," and "The Traffic in Halle Orphanage Medications." Five chapters deal with German medicine in North America from 1730 to 1810. The roles of practitioners, patients, and Pietist ministers, as well as women's activities and pharmaceutical trends like chemiatri and polypharmacy, are reconstructed, and the economic data is thoroughly evaluated. Thus Wilson finds "that the pharmaceutical trade was an indispensable medium through which the Halle Pietists transmitted financial support to their North American diaspora and reinforced its evangelical mission" (p. 211).

For the continental European reader it is somewhat surprising that Wilson sees in the Halle foundation of the eighteenth century a generic model "of the private, voluntary, and non-profit organization" of later epochs (p. 220), mainly dedicated to charity. To me, the manu-

factory seems to have been largely mercantilist and organized in a cameralist fashion. It was illegal under Prussian legal standards (Preußisches Medicinal-Edict von 1725), because secret remedies that were dispensed without the sanction of a pharmacist and priced without control by state authorities were forbidden, a public health measure that was part of the enlightened *Medizinalpolizei*. Also, the Halle medications were not really different from the usual polypragmatic and profuse semichemiatric *materia medica* of the period as represented in the *Pharmacopoea Wirttenbergica* of 1741, the most authoritative compendium of the time. Even within the century-long movement after 1680 for rationalizing and reducing the huge number of drugs and medicinal formulas, the Halle medications did not play a significant role (see Jochen Kühn, *Untersuchungen zur Arzneischatzverringerung in Deutschland um 1800* [Deutscher Apothekerverlag, 1976]). In the realm of proprietary and secret remedies the Halle medications were not unique: the *Olitäten*, from Thüringen, were predecessors in Central Europe, but without the aura of piety (Sabine Bernschneider-Reif, *Laboranten, Destillatores, Balsamträger* [Lang, 2001]).

An enthusiasm for Pietist, philanthropic, and voluntarist concerns and for the positive sides of entrepreneurship prevents Wilson from judging the secret medicaments trade more critically. In 1810, the foremost officer in the Prussian health administration during the reform period after the Napoleonic wars, Johann Gottfried Langermann (1768–1832), wrote in a legal brief: “The Halle Orphanage was not exempted from the provisions of the ‘Medizinal Edict’ of 1725, nor has it ever been since. The fact that sales of medicines occurred at the orphanage was due only to toleration out of negligence, favoritism, and connections, the doctors at Halle in particular being possessed by greed in distributing their arcana and nostra [i.e., proprietary medicines]” (Deutsches Zentralarchiv [former Preußisches Geheimes Staatsarchiv], Berlin-Dahlem, Histor. Abt. II, Rep. 76 VIIIA, Nr. 2115, fol. 23).

Thus it remains for future historians to judge whether the Halle orphanage’s worldwide traffic in medications was the earliest case of large-scale deception and exploitation in the marketing of medicines or, rather, a pious effort “for the well-being of Halle-American clergy” and a “linkage of straight charity and commercial activity for a charitable cause” (p. 117).

ERIKA HICKEL

Guido Canziani; Winfried Schröder; Francisco Socas (Editors). *Cymbalum mundi sive*

symbolum sapientiae: Edizione critica. (Filosofia e Scienza nel Cinquecento e nel Seicento.) 309 pp., illus., bibl., index. Milan: FrancoAngeli, 2000.

The publication of this definitive critical edition is a major cause for celebration by all students of seventeenth- and eighteenth-century philosophy, especially those concerned with the clandestine tradition feeding into atheism, skepticism, and empiricism. For the very first time this anonymous Latin treatise, composed about three hundred years ago and extant in seventeen differing manuscripts originating almost exclusively from Germany and central Europe, is available in an inexpensive, superbly edited paperback. Winfried Schröder, who has written on the origins of atheism and on Spinoza and the German Enlightenment and has brought out a critical edition of a version of *The Three Imposters* he attributes to J. J. Müller (1999), here discusses persuasively the date, authorship, title, historical context, and reception of this treatise. Francisco Socas, a philologist, describes all the manuscripts and their contents and establishes two versions of the text distinct in their authorship and date. (In the text, Version 2 is made plain to the reader by the use of bold print.) Guido Canziani, who critically edited the earlier anonymous Latin atheistic treatise *Theophrastus redivivus* and is the author of numerous essays on clandestine literature and philosophy in the seventeenth and eighteenth centuries, has provided an illuminating guide to the main themes and arguments. The notes, full of quotations from ancient and modern sources pertinent to the text, as well as discussions of its cultural context, are an education in themselves. All the material supplementary to the text is in Italian.

Many questions surround the work, beginning with the title: while “*Cymbalum mundi*” recalls the skeptical Lucianic dialogues written in French by Bonaventure Des Périers, printed in 1537, which soon disappeared from circulation, “*Symbolum sapientiae*” (this “wisdom” is rational, however, based on an understanding of human nature, and not religious) appears on older manuscripts. There are two main authors. Version 1, written after 1692, is attributed to Georg Michael Heber (1652–1702), a professor of law at Wittenberg who was conversant with theological and philosophical debates in Lutheran universities. The more certain author of Version 2, who was working on it between 1706 and 1720, adding significantly to the critique of religion and Scripture, was Johann Georg Wach-

ter from Leipzig, a follower of Spinoza and the author of several antireligious tracts.

The diffusion of *Cymbalum mundi* was not widespread outside circles of freethinkers in eighteenth-century Germany, apart from the pantheist Johann Christian Edelmann. It may be that the Latin, meant originally to restrict its readership to a learned elite, later impeded its spread among Enlightenment readers in France and England, no longer used to Latin for philosophy.

Spinoza's presence dominates all four sections of the *Cymbalum*. The fourth section, on moral philosophy, takes its cue from Hobbes—the title is “De origine boni et mali ex doctrina Hobbesii, ubi de origine societatum”—in denying that there are divine commandments revealed by God that provide the paradigm of human law. If anyone says that God has spoken about morality, the doctrines are fictions, “imposturae sunt” (p. 271), uttered by society's masters to press human minds into servile obedience. But in contrast to Hobbes, who justifies absolutism, the authors—here following Spinoza—praise freedom and the absence of hierarchy as supreme values. As Nature has given us the capacity to do whatever we wish (“libera facultas agendi quicquid lubet”; p. 268), the state has no right to bind humankind—a stance against Grotius's *De iure belli ac pacis*.

In the other sections on religion, God, and the Bible, “moderns” like Cornelius Agrippa, Bodin, and Herbert of Cherbury are used with approval against Descartes. Favored ancient writers are the Epicurean Lucretius and the hard-line skeptic Sextus Empiricus. The authors follow the learned libertine line about the three main revealed religions—Judaism, Christianity, and Islam—being instruments of political and social control over a credulous populace. Jesus is allowed to be an ethical philosopher appealing to reason and liberty against laws and rituals, but not a divine person. Indeed, the institutionalized churches, especially Catholicism, have betrayed his message for their own worldly gain. The section on the Bible also follows the libertine line that miracles and prophecies are either tricks perpetrated by priests or natural phenomena; it establishes a rational approach to biblical studies by the use of historical and philological methods applied since the Renaissance to classical texts.

The section on God is perhaps more rigorously philosophical than the others, examining and refuting eleven proofs for the existence of God to arrive at the agnostic and skeptical conclusion that while one can refute the supposed proofs, one cannot conclusively prove the non-existence of a supreme being. The “political” ar-

gument for religion—that all morality would collapse if there were no God, and that consequently belief in God, like the buttresses on medieval cathedrals, preserves law and order—finds no sympathy. Good and evil, the just and unjust, depend not on any law of God but on human beings themselves (p. 244). An English translation of the text, the notes, and the introductory material is highly desirable.

LETIZIA PANIZZA

Jorge Cañizares-Esguerra. *How to Write the History of the New World: Historiographies, Epistemologies, and Identities in the Eighteenth-Century Atlantic World*. 449 pp., illus., notes, bibl., index. Stanford, Calif.: Stanford University Press, 2001. \$55 (cloth).

This is a powerful book. It deals in a masterful way with a corpus of evidence comparatively underused in the history of the Enlightenment and demonstrates its leverage on central Enlightenment problems. It also finds some new approaches to that well-worked area. Historians—Spanish, Creole, and Amerindian—between the sixteenth and eighteenth centuries in Spain and her New World possessions attempted to construct a viable history of their colonial past. To do so, they had to confront the problem of the meaning, interpretation, and reliability not only of sources produced by Spanish historians of the conquest of the Americas and later periods but also Amerindian chronicles relating to both the conquest itself and far earlier periods. How were these sources to be handled? Some were written in pictograms, using difficult chronology, or in Indian languages that had undergone such radical changes since contact with the Spanish that their interpretation was doubtful. Which were the more veracious and why—those written by eyewitnesses to the events they narrated or those written by Indians or Spaniards of high social status? Were the Indian texts expressed in pictograms to be treated as records of real history or, as they were for the Enlightenment, as merely visual representations of interest only as exemplifying the history of the development of the human mind?

In answering such questions, Jorge Cañizares-Esguerra has demonstrated levels of technical competence well beyond those normally demanded in the contemporary profession. He has had to track down and collate manuscript and printed sources from Mexico City to Vienna. He has unraveled archival puzzles and carefully checked the footnotes and attributions of previous scholars, sometimes with surprising results.

He has done more than this, however. He has gone beyond the questions asked in Anthony Grafton's *Defenders of the Text* (Harvard, 1991) about the creation of chronologies and techniques of historical interpretation or about how early modern historians dealt with the "deep time" of the Egyptian mysteries. Grafton dealt with these questions largely in terms of the unfolding of the practice of history. Cañizares-Esguerra links the unfolding of the practice of history to the nature of the Enlightenment.

His Enlightenment spans the Atlantic, from Peninsular Spain to its colonies in the Americas, taking in the Scotland of William Robertson's *History of America* (1777) and *History of Charles V* (1769) and his links with the Spanish royal minister Pedro Rodríguez, count of Campomanes, and proceeds to G.-T.-F. Raynal. But the focus of the book is the relationship between Peninsular and Creole historians, and between all these historians and the problematic creation of their own pasts. The book treats with unduly gentle criticism some assertions in my own volume *The Enlightenment* (Cambridge, 1995) about the Enlightenment as a global phenomenon whose common intellectual agendas overrode social, economic, and political differences in Europe and its colonies. As Cañizares-Esguerra observes, "the language and rules of northern European discourses and controversies were not passively transferred to the New World, Spain, or the Papal States. The themes that were significant to the public in Spanish America had less to do with building new religious and political languages than with constructing alternative, critical epistemologies. The Spanish American Enlightenment was a dual process of creating such discursive space and consolidating a public sphere" (p. 260). Such statements, and their substantiation, remind us of the pressure placed by colonies on metropolitan Enlightenments, precisely because the specificities of history and landscape, and of the incorporation of indigenous peoples, faced the Enlightenment with one of its biggest and least well-solved problems, that of the understanding of difference. It is in this way that the paradoxical structures of the Enlightenment anticipate the structures of modern globalism, where the local and the global are powerfully interlocked and yet pull away from each other, like evenly balanced wrestlers.

Finally, this volume is of considerable interest to all those, including historians of science, who work on historical epistemology. Fact and value, truth and evidence, witnessing and interpretation are central problems wrestled with by Peninsular

and Creole historians alike. They are—paradoxically, in relation to the author's other frameworks—also those that were common to many Enlightenment inquiries. But then paradox is the heart of good history, just as it is the heart of Enlightenment itself.

DORINDA OUTRAM

Nina Reid-Maroney. *Philadelphia's Enlightenment, 1740–1800: Kingdom of Christ, Empire of Reason.* (Contributions to the Study of World History, 81.) xvi + 199 pp., illus., bibl., index. Westport, Conn./London: Greenwood Press, 2001. \$62.50 (cloth).

Philadelphia in the second half of the eighteenth century was the cultural and, for a time, the political capital of America: the center of the American Enlightenment. With a population of little more than thirty thousand souls, it could boast of four prestigious institutions of learning: the American Philosophical Society, the College of Philadelphia, the College of Physicians, and the Pennsylvania Hospital; one could add, although Nina Reid-Maroney for her purposes does not, Peale's museum of science and art. The men managing these institutions form a glittering array of intellectual talent and have come to be referred to as "the Philadelphia circle." These considerations alone would make Reid-Maroney's monograph an important book; more significantly, this study gives us, at long last, a necessary corrective to previous interpretations that have made the American Enlightenment either a pragmatic episode in which Americans "practiced" what Europeans only theorized or an intellectual movement dominated by "reason" and deism. Reid-Maroney's trenchant profiles of Benjamin Rush, John Redman, Francis Alison, John and William Bartram, Samuel Stanhope Smith, and others in the Philadelphia circle show us how these men successfully fused the new science to the Calvinist doctrine of God's grace and the New Side theology of the Great Awakening. (One could add Charles Willson Peale, John Beale Bordley, David Rittenhouse, and Ebenezer Hazard, but they do not, again, fit Reid-Maroney's purposes.) By focusing on this circle of men, she is able to modify Henry May's Anglican, Arminian template, which gave us an uneven balance between a dynamic and robust scientific knowledge and a pale liberal theology in, at best, graceful decline. Reid-Maroney makes clear that a fervent and intellectually acute religion was integral to Philadelphia's Enlightenment. The men of the Philadelphia circle were not Thomas Jefferson writ large. She em-

phasizes two similarities in their backgrounds. Almost all of them were schooled in New Style Presbyterian seminaries that gave them a clear image of a Calvinist God who was a central and constant operator in history. Also, the many doctors in the circle were trained at the Edinburgh Medical School by such men as William Cullen, whose skepticism regarding scientific theory complemented Calvinist belief regarding the insufficiency of reason unaided by grace; to this mixture they added the doctrine of man's intuitive moral "common sense" from the Scottish Enlightenment. Their Newtonian universe did not relegate God to a mere clockmaker who left the cosmos to run by its own mechanical laws; rather, all forces in nature were but visible manifestations of grace, the "energy of God."

Reid-Maroney loses her sure footing only in her concluding pages. Like every scholar of American religion, she must confront the phenomenon of declension: in this case, the unraveling of the synthesis of science and religion in Philadelphia's Enlightenment. Perry Miller, who had to account for a hegemonic shift in New England society and religion, had a far larger problem. Unlike Miller's, Reid-Maroney's account does not utilize such major transformations in eighteenth-century America as the commercial and consumer revolutions, the maturation of society, and the professionalization of science. Instead, she hints at a "gendered" construction of religion and science in which the skepticism and intuition present in the Enlightenment are "feminine" and empirical advances in science are "masculine." The "masculine" science overcomes the "feminine" synthesis of the Enlightenment. But Occam's razor demands that we first deal with direct and major causes. One hundred years from now historians will still be debating modernization, long after they have abandoned such tendentious gendered constructions.

SIDNEY HART

Margaret Healy. *Fictions of Disease in Early Modern England: Bodies, Plagues, and Politics.* xii + 277 pp., notes, index. New York: Palgrave, 2002. \$62 (cloth).

Margaret Healy's fine book looks at the complex dynamic between disease and literature in sixteenth- and seventeenth-century England. While medicine and the body provided a plethora of metaphors for the literature of this era, Healy argues that this was not a one-way exchange: medicine and literature constituted a series of intertwining and interpenetrating discourses. In *Fictions of Disease in Early Modern England*

she untangles some of these and reveals a wide range of social concerns not only about disease and the body but also about the body politic. In this period of great epidemics and great political upheavals, medicine and society shared discourses of the plague and the pox, of excess and of wasting away.

Healy had a career as a health-care worker before she became a professor of English literature, and she looks at medical theories and sick people with the same attention she gives to Shakespeare and Milton. As she points out, too many works on literature and medicine in this period assume an overly simplistic view of medical theories. Healy's own wonderfully nuanced exploration of medical theories, as presented in books of regimen, fully reveals the complex and at times contradictory theories of the period, when the old Galenic paradigm coexisted with Paracelsian ideas as well as with the anatomical view of the body expounded by Vesalius. Her analysis of books of regimen shows the wide range of available explanations as well as how these explanations changed over time, effectively exploding the common view that medical theories (and practices) changed very little until the eighteenth century.

As Healy notes, the Reformation increasingly added religious (and occult and moral) discourses to medical writing; these overlapped and competed with natural arguments. Writings about the plague show all of these explanations, along with use of the plague as a metaphor for the sickness of society. What Healy calls "this eclectic soup of competing and complementary narratives" "shapes the cultural imaginary and ultimately determines the ideological appropriations of bubonic plague" (p. 51). She shows how plague pamphlets, particularly those of the playwright Thomas Dekker, reveal these layers of meaning and the critique of society embedded in the discourses of plague.

Healy performs a similar task for writings on the "pox," or syphilis. Here she turns to both literary and medical writings, ranging from Erasmus's *Colloquies* to the plays of Shakespeare and Jonson, to make her case. She compares the literary treatment of the pox to contemporary accounts of AIDS, noting in both cases the mixture of medicine and morality and the predominance of "cultural myths" (p. 153) about the disease's origins. The pox, even more than the plague, lent itself to moralizing commentary that elided individual sin with the corruption of society as a whole. Thomas Dekker again plays a prominent role, in this case with his *Honest Whore* plays.

In the final chapter Healy discusses gluttony, widely perceived in this era to be a major cause of disease and of course also a major source of metaphors about the body politic. In plays, economic tracts, court masques, and medical books of regimen, excessive consumption caused moral as well as bodily decay. In Milton's *Paradise Lost* regicide is ultimately justified as the consequence of a royal culture of excess.

Healy's book is an excellent and rich commentary on the multiple narratives of medicine in this era. Her writing style is clear, straightforward, and free of jargon. She gives us a new and very fruitful model for integrating the history of science and medicine with literary analysis.

ANITA GUERRINI

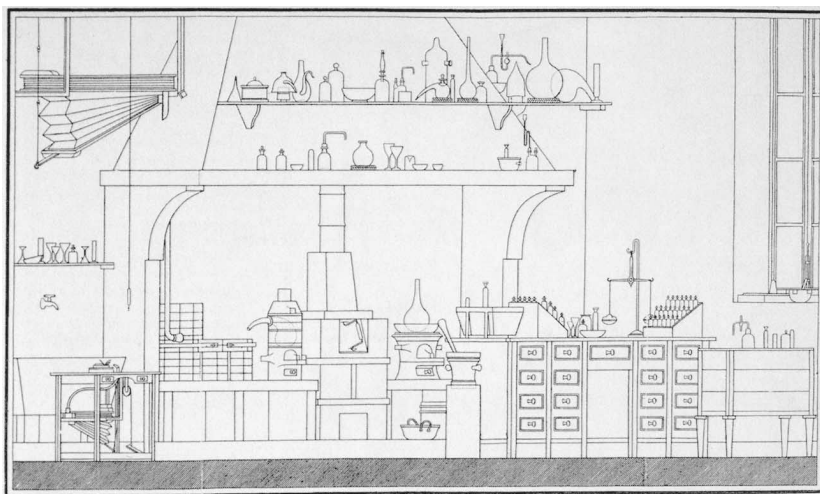
Jose M. López Piñero; Víctor Navarro; María Luz López; José R. Bertomeu; Antonio García Belmar; Felipe Jerez. *La actividad científica valenciana de la Ilustración*. 2 volumes. 254 + 230 pp., illus., bibls., indexes. Valencia: Diputacio de Valencia, 1998. (Cloth.)

José María López Piñero is justly considered the founding father of historical studies on science in Spain. He has been the undisputed master of several generations of historians, and his pioneering study *Ciencia y técnica en la sociedad española de los siglos XVI y XVII* (Labor, 1979) was the textbook that established the guidelines for studying scientific activities in early modern Spain. López Piñero has been a professor at the

Consejo Superior de la Ciencia and the University of Valencia. Medicine, bibliometrical studies, and culture in the Valencia region have always been among his topics of research. *La actividad científica valenciana de la Ilustración* is one of the last products of his long and fruitful career and a good representation of his school, for the rest of the authors are or have been members of the department he created, including both representatives of the first generation (e.g., Víctor Navarro) and relative newcomers (Antonio García Belmar, José Bertomeu).

The two lavish volumes are divided in four parts. The first part is devoted to the historical study itself. The first volume concludes with a set of Valencian scientific leaflets and books produced between 1700 and 1814 and a selection of plates and illustrations. The reproductions are of an outstanding quality. The second volume contains a register of scientific engravings that were included in books written, translated, or simply stocked in Valencian libraries during the eighteenth century. This volume has no graphic images, just references. It will be extremely useful for researchers seeking biographies of and data on the numerous painters, engravers, illustrators, authors, publishers, and printers who were involved in the spread of Enlightenment culture in Spain.

Contrary to the stereotype generated in northern Europe in the eighteenth century, Spain enjoyed a vast scientific culture before, during, and



Spanish chemical laboratory circa 1804 illustrated in a chemistry textbook (from López Piñero et al., La actividad científica valenciana de la Ilustración, p. 228).

after the time of the Royal Society and the *Encyclopédie*. The absence of the Iberian Peninsula from the so-called Scientific Revolution, the Counter-Reformation, and the long decline of the Spanish Empire have led to the widespread belief, shared throughout the Anglo-Saxon world and especially by those who consider France the true center of the Enlightenment, that Spain was a peripheral region, out of the European scientific mainstream. Spain simply was not a part of the Great Tradition, did not show up in the Big Picture. The Spanish Enlightenment was, according to that view, a late, imported, and mimetic phenomenon—an attitude that can be recognized in the old but very influential works of Hispanicists such as Jean Sarrailh and Richard Herr.

All these reasons lead me to think that this book might be more interesting for non-Spanish readers than for Spanish scholars. López Piñero and Navarro emphasize arguments already well known in their specialized circles but still out of step with current Anglo-American trends. A look through the table of contents of Roy Porter and Mikuláš Teich's *The Enlightenment in National Context* (Cambridge, 1981) or William Clark, Jan Golinski, and Simon Schaffer's *The Sciences in Enlightened Europe* (Chicago, 1999)—to name just two excellent books—will suffice to indicate that for Cambridge and the University of Chicago Press “south” means Italy.

Now, when the concept of the Scientific Revolution, the cartography of the Enlightenment, and the prehistory of objectivity are all being seriously revisited, it is time to include new realities in those new perspectives, shading in the areas of the old Big Picture to accommodate the Catholic and syncretic cultures of the Enlightenment that run from Valencia to the Philippine Islands and from New Spain to the Strait of Magellan.

JUAN PIMENTEL

René Sigrist (Editor). *H.-B. de Saussure (1740–1799): Un regard sur la terre*. (Bibliothèque d'Histoire des Sciences.) x + 541 pp., illus., tables, apps., index. Geneva/Paris: Editions Médecines & Hygiène, 2001. Fr 220 (paper).

The latest production from the cottage industry devoted to Horace-Bénédict de Saussure is an impressive tome. Its twenty-four distinct chapters, by a multitude of authors, succeed in portraying Saussure as a complex and multifaceted person, emblematic of the aristocratic natural scientist of the Age of Enlightenment. *H.-B. de Saussure (1740–1799): Un regard sur la terre*

presents a wide-ranging and frequently in-depth account of Saussure's life and times. Three major sections, each introduced by René Sigrist, the editor, consider Saussure's contributions as a scientist, specific aspects of his *Voyages dans les Alpes*, and elements of his life, from his educational views to his difficulties with health and wealth. Although not, by design, a seamless biography, the book provides many rich insights into eighteenth-century science and letters, as practiced by a well-traveled gentleman of means with deep roots in the milieu of Geneva and the surrounding Alps.

Even those who recognize Saussure as a legend in mountaineering, conqueror of Mont Blanc in 1787, and a significant figure in the basalt controversy may be surprised at the range of his contributions to science. The first section of the book considers his work in botany, physics, the study of microscopic “animalcules,” meteorology, glaciology, volcanology, structural geology, and physical geography. These “Itineraries of a Researcher” take up about half the book. Philosophers and historians of geology are likely to be struck by Saussure's firmly inductive epistemology, built on detailed observation and rigorous measurement. In fact, he designed or improved some of the equipment, such as hygrometers and cyanometers, used to make meteorological measurements. Authors of the ten chapters in this section make it abundantly evident that Saussure's interests were interdisciplinary but transcended dilettantish dabbling. He made observations and field measurements from alpine mountains to Italian volcanoes, and he used state-of-the-art meteorological instruments or microscopes set up in his own elegant mansion.

In the second section, six chapters revolve around *Voyages dans les Alpes*. Sigrist introduces the discussions by providing a historical context for Saussure's innovative scientific travelogue through the beautiful but then somewhat terrifying Alps. He notes that in the *Voyages* we see the complex mix of scientific concepts, descriptions of topography and biota, and even personal or emotional responses that made the work alluring to a wide readership. Intersections between literature and science are evident in Saussure's use of iconography, anecdote, and aesthetic description. The question is raised as to whether Saussure was fundamentally a scientist or a voyager—or both.

The concluding section, comprising seven chapters, considers Saussure as an individual representative of his time, place, and status. Specific topics include the formative role of his

agronomist father, Horace-Bénédict's plans for reforming the educational system in Geneva, his involvement with architectural issues, and his participation in the Société des Arts. Personal details of his life receive attention in chapters on his spirituality, his health, and how he lost much of his fortune. A mere listing of the subject matter does not do justice to the richness of the narrative and the wealth of information contained in virtually all the chapters of the book.

Nor does a discussion of the text adequately convey the impact of the illustrations, many in vibrant color. A synergism of text and illustration exists, in which works of art, cartoons, diagrams, photographs, original sketches, and tables are nicely integrated with the commentary. Fiery erupting volcanoes, striking portraits, and mountain-climbing parties (Saussure in eye-catching red) compete for attention with excellent black-and-white illustrations of landscapes, geologic structures, and instruments and relevant charts. It seems that the authors and editors expended considerable effort in producing a well-illustrated compendium of many key aspects of Saussure's life and times.

Also commendable, in terms of production, are the lack of typographic miscues and the excellence of the paper, printing, and reproduction. A cover-to-cover reading did not reveal a single egregious or entertaining typo. The index, alas, is limited to proper names, but one appendix presents key dates in H.-B. de Saussure's life and another incorporates a bibliographic summary. Fans of Saussure will applaud this informative and superbly produced book.

KENNARD B. BORK

Ter Ellingson. *The Myth of the Noble Savage*. xxii + 445 pp., illus., bibl., index. Berkeley/Los Angeles: University of California Press, 2001. \$60.

The Myth of the Noble Savage begins with the disclaimer that Jean-Jacques Rousseau, widely accepted as the creator of the concept of the noble savage, did not originate the phrase or ever use it in his work. Instead, Ter Ellingson discovers, the first use of the concept of savage nobility was in a description by Marc Lescarbot of the Mic Mac of Canada, published in 1609; the term "noble savage" appeared in a play by John Dryden in 1672 and then disappeared until resurrected as a derogatory phrase by John Crawford, president of the Ethnological Society of London, in 1859.

By carefully examining a significant body of ethnographic literature, Ellingson accomplishes

several objectives. He traces the history of ethnography and the development of the modern field of anthropology, showing how the rhetoric of the noble savage evolved from early seventeenth-century intellectual debates over the nature of human civilization to an often-virulent assertion of racial categories in the second half of the nineteenth century, an assertion that made the white race superior to all others. He also traces the power of the phrase as it was used in the late 1800s to juxtapose savagery and nobility as opposing categories for political purposes. Finally, he shows the continuing pervasiveness and power of the phrase in contemporary American society, discussing its use on the Internet and with respect to opposition to the 1999 killing of a gray whale by a group of Makah Indians as an assertion of their rights under a treaty signed with the U.S. government in 1855.

The intellectual journey of the noble savage takes the reader through the works of early travelers, including Baron de Lahontan (1703), Pierre de Charlevoix (1744), Joseph-François Lafitau (1724), and Carl von Linnaeus (1732). Ellingson comments on the development of an observational method and a growing scientific objectivity toward native people that came into being in the Enlightenment. He also examines the works of American observers, primarily George Catlin, whose paintings provided visual representation to accompany his extensive descriptions of Indian customs, and Lewis Henry Morgan, generally considered the founder of the modern discipline of anthropology because of his extensive collections of documentary evidence of kinship terminology and his theory of the evolution of human cultures.

Ellingson devotes the last two thirds of the book to a consideration of "Popular Views of the Savage" (the title of Ch. 11) and the emergence of the racist anthropology that was promoted most strongly by James Hunt, a member of the Ethnological Society of London and a strong polemicist for the superiority of the white race. Here Ellingson's narrative moves more toward an examination of the politics of science, describing a "disastrous meeting" (p. 272) of the Ethnological Society in 1858 or 1859 at which only six members were in attendance. Subsequent events allowed Hunt and Crawford to take control of the society—as secretary and president, respectively—and use it as a forum to promote their racist views.

The Myth of the Noble Savage is at its best when it explores the evolution of the idea of savage nobility along with the development of the fields of ethnology and anthropology. The ar-

guments concerning the development of racist anthropology are less compelling, perhaps because they are indeed based more on the politics of the idea than on its scientific merit, and Ellingson's extremely unflattering picture of James Hunt borders at times on the emotional rather than the objective.

On the whole, however, the book is a solid and scholarly treatment that, in showing the power of an extremely widely accepted idea, also shows how it has shaped the study of human societies over time.

CLARA SUE KIDWELL

Benjamin Schmidt. *Innocence Abroad: The Dutch Imagination and the New World, 1570–1670.* xxiv + 450 pp., illus., notes, bibl., index. Cambridge: Cambridge University Press, 2001. \$64.95 (cloth).

At first glance, the Dutch and the New World would hardly seem a worthy subject for so large a book. It was only after 1620 that New Netherland and the Dutch Brazilian settlements were founded, and their success was marred by economic failure and challenges from other European claimants. By 1670 Brazil had fallen to the Portuguese, New Netherland was transferred to

Charles II under the terms of the Treaty of Breda, and the Dutch West India Company (WIC) was bankrupt. It is in showing the relevance of this previously neglected subject that *Innocence Abroad* is to be admired. Benjamin Schmidt delves deeply into Dutch artwork and literature and finds that despite their limited presence in America, “the Dutch won the contest of *representing* a New World” (p. xix).

Schmidt argues that Dutch discourse about the New World—facilitated by the presence of some of the most productive printing presses in Europe—became vital propaganda in the years of resistance to Hapsburg rule (1560s–1590s). The Dutch exaggerated the plight of the American natives, representing them as kindred spirits also tyrannized by the Spanish and fashioning “a version of America that matched the rhetorical imperative of the day” (p. xix). These “cultural geographers” turned the Spanish into colonial warlords; the “Other” was not the American natives or the New World but the Catholic and jingoistic Spanish. Accordingly, when the Dutch entered America in the early decades of the seventeenth century, they desired to grant the natives their natural liberties of freedom of religion and trade. This attitude quickly became tyrannical as the WIC attempted to establish its dom-



A “Human Abattoir,” from a Dutch translation of Bartolome de las Casas, *Den Spiegel der Spaensche tyrannye gheschiedt in West Indien* (from Schmidt, *Innocence Abroad*, 1570–1670, p. 120).

inance. After 1650, representations showed America as a breeding ground of Dutch tyranny, decadence, and corruptibility. For a century, depending on the political moment, Dutch representations of America vacillated between “the twin topoi of ‘innocence’ and ‘tyranny,’” which could “withstand an impressive amount of twisting and inversion” (p. 313). Schmidt concludes that “the centrality of America within Dutch discourse adds a new dimension to the study of the Revolt and Eighty Years’ War, and enriches our understanding of Dutch culture in the Golden Age” (p. 315). That is, contrary to the “blunted impact” thesis of John Eliot, at least in the case of the Netherlands America *did* impact the European consciousness.

Schmidt is good at describing the printing and reading communities and the political scene of the Netherlands. This is important in a book whose central thesis relies on the dissemination of information and the political moment. He defines “cultural geography” as “the manner in which other places and peoples were imagined, appropriated, and manipulated” (p. xviii) for social, economic, and cultural reasons and claims that his is “a fresh approach to the history of early modern geography, which might be studied not simply to gauge the advances in mathematical or scientific representation” (p. 315). But although the sources and arguments are original and important, the approach is less so. A better indication of his methodological predecessors and some refinement of his rubric term are necessary in light of recent works on social, political, and descriptive geography, chorography, and cartography (e.g., Lesley Cormack, *Charting an Empire* [Chicago, 1997]; and the collections of previously published essays in Andrew Gordon and Bernhard Klein, eds., *Literature, Mapping, and the Politics of Space in Early Modern Britain* [Cambridge, 2001], and J. B. Harley, *The New Nature of Maps: Essays in the History of Cartography* [Johns Hopkins, 2001]). The exclusion of Dutch maps of America is surprising, given the rich mapmaking culture of the Netherlands and their relevance to the broader thesis. Schmidt has, fortunately, addressed this topic briefly elsewhere (*William and Mary Quarterly*, 3rd ser., 1997, 54:549–578). On the whole, he has produced a valuable, coherent, and often elegant and entertaining text that will appeal to scholars in a wide variety of disciplines.

KEN MACMILLAN

Jonathan Andrews; Andrew Scull. *Undertaker of the Mind: John Monro and Mad-Doctoring in Eighteenth-Century England*. xxii + 389 pp., il-

lus., notes, bibl., index. Berkeley: University of California Press, 2001. \$35 (cloth).

Jonathan Andrews and Andrew Scull’s gripping biography of John Monro, the visiting physician to London’s Bethlehem (“Bedlam”) Hospital for forty years, owes its generation to the authors’ discovery of Monro’s private casebook of 1766—an artifact previously known only to his descendants. Through this casebook, the authors have sought insights, somewhat obsessively, into the “mad-business” as represented through the perspectives of Monro, his colleagues, and their patients. Their efforts have since expanded beyond this one treasured resource, culminating in *Undertaker of the Mind: John Monro and Mad-Doctoring in Eighteenth-Century England*. This delightful book traces the displaced Scotsman from his Oxford education, to an apprenticeship with his “mad doctor” father, James Monro, through many career highlights as one of England’s premier specialist physicians.

Drawing extensively on epigraphs, satirical verse, and iconography, the authors succeed in their quest to demonstrate how “the sociocultural setting of an individual’s life is at least as significant as the individual’s subjectivity and particular personality traits” (p. xiii). One early chapter emphasizes this sociocultural setting as it attempts to right the balance between Monro and William Battie. Typically, Monro has been characterized as the conservative reactionary to his progressive, enlightened (and victorious) polemical and institutional rival, William Battie, physician to St. Luke’s Hospital for Lunatics. Building on the medical historian Akihito Suzuki’s reassessment of Monro in light of his eighteenth-century peers, the authors depict Battie’s changing relationship with Monro from the time he once openly criticized Monro for attempting to monopolize madness to the development of a “mutually supportive association” (p. 71) between them.

In another chapter, the intertwined activities of religious enthusiasm and manifestations of madness are unraveled. Monro’s hostility to sectarian religions, especially to the Methodists, who promoted behaviors viewed as inciting both civil and mental unrest, are explored. The religious enthusiasm and asylum confinement of Alexander Cruden, author of the influential *Complete Concordance to the Old and New Testaments* (1737), is examined in detail. Cruden’s eloquent and detailed narrative of his own confinement is ably used by Andrews and Scull to reconstruct encounters of the mad at the hands of Monro. The recognition of sanity regained, a

prognostic possibility peculiar to the privileged, is shown through an analysis of Monro's "successful" treatment of George Walpole, the third earl of Orford and grandson of Prime Minister Robert Walpole. Not all nobility, however, were successfully treated by Monro. In one case, Monro's expert testimony favoring temporary insanity failed to save the accused murderer Laurence Shirley, earl Ferrers, from the gibbet. Monro's difficulty in discerning temporary from chronic insanity also became a focal point in his deliberations over the mental state of Margaret ("Mad Meg") Nicholson, the would-be assassin of George III and eventual lifer in Bedlam.

Andrews and Scull's perceptive portrayal of Monro as an "undertaker of the mind" exemplifies their argument that in caring for "the darkened shadow of human fears, associations, and motivations," the mad-doctors were "engaged in activities that closely paralleled the fashion in which undertakers made their living from the corruption and death of the body" (p. xix). This is particularly apparent as the authors explore the underlying structural identity of madness through Monro's case reports, his patients, the Grub Street press, legal statutes, and the general architecture of madhouses. The increasingly discussed and dispersed societal imagery of madness in the eighteenth century appears to have bolstered the business of lunacy—a market in which Monro reaped considerable reward and reputation. The authors' focus on the parallels between caretakers of the mind and body proceeds far beyond the level of metaphor. Indeed, it provokes further investigation into the extent to which Enlightenment practitioners, in England and elsewhere, truly perceived the body and the mind as separable entities requiring specialized treatment.

PHILIP K. WILSON

John Zammuto. *Kant, Herder, and the Birth of Anthropology*. 480 pp., illus., index. Chicago: University of Chicago Press, 2002. \$29 (paper).

By the mid-eighteenth century, according to Pierre Cabanis, writing in 1796, Germans were comfortable using the term "anthropology," "but the French (and British) had long before been active in cultivating the thing" (p. 221). Traditions of physiological psychology, which were largely the province of medical theorists; of philosophical treatments of the relations between mind and body; of natural history, which dealt with the relationship of humans to other animals; of conjectural history, which explored human societies in relation to their environments and

modes of production; of aesthetics, which sought to understand the foundations of taste and of the feelings of beauty and sublimity, all pervaded eighteenth-century discourse. But it was not until a series of events occurred in Germany at the beginning of the 1770s that anthropology became established as an autonomous academic discipline. Johann Gottfried Herder published his *Essay on the Origin of Language* in 1771 in response to a Berlin Academy prize competition; Immanuel Kant initiated a separate course on anthropology at Königsberg in 1772; and Ernst Platner published a widely used textbook, *Anthropologie für Ärzte und Weltweise*, in 1772.

John Zammuto's principal goals in this work are to explain the circumstances that led to the creation of Kant's course in 1772; to explore why Kant's approach to anthropology in that course was radically different from the approaches taken by Platner and by Kant's one-time protégé, Herder; to argue that Herder's approach was an outgrowth of the way that Kant had been thinking and teaching during the 1760s; to defend Herder's work against Kant's criticisms that it was superficial; and to argue that Herder's approach to anthropology was more important in establishing the discipline than that of his mentor. Along the way, Zammuto involves us in a reevaluation of a number of Kant's precritical writings, linking Kant's early career to a tradition of *Popularphilosophie* that was bitterly opposed to the rationalist *Schulphilosophie* associated with the followers of Christian Wolff. He reassesses the particular ways in which both Rousseau and Hume were central to the development of Kantian thought. Moreover, he offers a sketch of the various European traditions of anthropological thought from which both Kant and Herder drew.

Zammuto offers a comprehensive and compelling argument that Kant was repelled by the way in which academic philosophy—especially metaphysics—was taught in Germany during the mid-eighteenth century, that he aligned himself with those who sought to teach a practical philosophy for active citizens at least up to 1769, but that he remained obsessed throughout his life with the notion that a correctly done metaphysics was of paramount importance even for practical philosophy. Zammuto makes it clear why, once Kant had initiated his critical philosophy and confirmed the possibility of establishing certain metaphysical principles *a priori*, he should have restructured his metaphysics courses, eliminating the empirical psychology that he and others, following Hume, had used to ground their introductory philosophy courses. That material, along

with material that had previously been incorporated into a physical geography course, was now integrated into an anthropology course, which was subordinated to his new metaphysical principles—principles that denied the very possibility of a physiological psychology.

Zammito shows that Herder, on the other hand, never abandoned the commitment to *Popularphilosophie* that had informed Kant's pre-critical writing and teaching. Herder continued to hope for the reduction of metaphysics to anthropology. He was especially concerned with aesthetic issues, and, like Platner, he grounded his anthropology in a physiological psychology whose legitimacy Kant's critical philosophy denied.

My major complaint about this book is that, in his attempt to be comprehensive, the author is at times confusing. At one point Zammito complains of Kant's autobiographical reflections, "He tells too many stories and they do not all converge" (p. 258). I feel much the same about Zammito's often fascinating, but not always clearly focused, reflections on the contexts for Kant's intellectual development.

RICHARD OLSON

■ Modern (Nineteenth Century to 1950)

Grenville Goodwin; Neil Goodwin. *The Apache Diaries: A Father-Son Journey*. xx + 284 pp., frontis., figs., apps., index. Lincoln: University of Nebraska Press, 2000. \$29.95.

Anthropology as adventure. A search for an elusive indigenous tribe, living under the harshest conditions, hiding in order to survive, harassed and feared by immigrants who slowly but steadily take over their territory; an anthropologist who wants to record their culture before it disappears. Travelers who endure dust, heat, rattlesnakes, bandits, corrupt officials, and revolutionaries as they head south from Tucson hoping to find the remnants of the once-strong warrior society. Feel the allure of northern Mexico, with its colorful cowboys, rugged individualists, outlaws, strong men and women. This is anthropology as quest for the vanishing primitive. Anthropology as experience that cannot be duplicated in the world of the bourgeoisie. Anthropology as the epic search for self in the pursuit of that most mysterious "other," the last of the "wild," dangerous, and dreaded Apache.

Enter the main characters: the ethnographer Grenville Goodwin, who worked with the Western Apache and died young, leaving unfinished

his attempt to contact the last free bands of Chiricahua Apache who had retreated into the remotest and most desolate parts of the Sierra Madre; and the filmmaker and historian Neil Goodwin, his son, who barely remembers the father who died when he was a baby yet yearns to know him. Neil decides to find the enigmatic man through his writing, a set of extensive diaries, and ethnographic explorations by completing Grenville's unfinished journey, producing in the process his own extensive and rich field journal. Enter the supporting characters: Neil's friends who accompany him on his journey of discovery and fulfillment; and the hundreds of fascinating people they meet as they travel over the same terrain, separated by fifty years. And in the distance are the fugitive Sierra Madre Apache, lurking behind rocks, silently watching, yet refusing to be seen or encountered on any but their own terms.

The Apache Diaries is a double quest narrative more than a reconstruction of an episode in the history of science or a biography of a scientist. It is told as a series of juxtaposed episodic vignettes that intertwine the two field diaries with the researchers' reconstruction of the Sierra Madre Apaches' experiences as part of a regional history. First, sections of Grenville's previously unpublished journals are presented. Each is followed by Neil's account of his own adventures as he attempts to complete his father's journey and simultaneously to uncover the emotional tenor of the meticulously neutral observer of the 1930s. The result is postmodern in intent and very gendered in application. This is a male quest tale, similar to those expeditions to find the earliest hominid and win the resultant academic prestige noted by Roger Lewin in *Bones of Contention* (Simon & Schuster, 1987).

In terms of the history of science, *The Apache Diaries* brings to light how 1920s–1930s ethnography and 1970s–1980s ethnohistory are often undertaken. It shows remarkably well the character of fieldnotes and the way regional and local history can be reconstructed through participant observation and extremely detailed, meticulous documentary research and oral narratives. It also captures the difficulty of searching for a truth, of trying to separate out prejudice and folklore from reality, of using research to engage in a dialogue with the past. These are articulated but understated concerns. Neil's real goal is to convey the feel of ethnographic and historical fieldwork, and this he does with panache.

In all, *The Apache Diaries* is a whopping good tale, worthy of an explorer who is a highly respected documentary filmmaker. But it is ulti-

mately sad. Sad because it documents the lives of the resourceful but beleaguered Sierra Madre Apache and because, in the end, the author who caught a glimpse of his father, learning much about his life and his quests for knowledge, could not quite grasp the man.

NANCY J. PAREZO

Edwin A. Abbott. *The Annotated Flatland: A Romance of Many Dimensions.* Introduction and notes by **Ian Stewart.** xxvii + 239 pp., illus., bibl., notes, refs. Cambridge, Mass.: Perseus Books Group, 2002. \$30 (cloth).

This is the latest exemplar of what is fast becoming a tradition of annotated classics. Ian Stewart's notes expand Edwin Abbott's book in many directions and put it in the context of works it arose from and begot, as well as in that of the mathematics that Abbott's fantasy employs.

There is a lot of fun here, and conceivably notes such as that on page 140 are part of it: there Stewart speaks of P. D. Ouspensky's influence on Dostoyevsky's *Brothers Karamazov*—remarkable, since Ouspensky was two years old (by the dates Stewart supplies) when the novel was written. Less funny, perhaps, is the sacrifice of historical accuracy to political correctness: Did British reprisals in the Sepoy Mutiny really far outweigh the actions that caused them (p. 94)? Did social pedigree really outweigh talent, intelligence, and ability for the Victorians (p. 101)? Tell that to the Lunar Society, Joseph Paxton, Chamberlain and company. Stewart's claim that "social Darwinism" appealed to the Victorians "because it provided a biological justification for an inequitable society" (p. 45) is exactly wrong: Andrew Carnegie, for example, gave his wealth to public libraries to save his children from privilege and passively to aid those who sought to rise.

The scholarship could be better. Accents and breathings are missing from Greek passages (p. 3) and "*kata*" is mistakenly translated as "against" (p. 27). Abbott is spoken of as "competent" in mathematics (p. 116) and as having passed the mathematical tripos at Cambridge (p. xviii), when in fact he took the highest honors in it (*Encyclopedia Britannica*, 13th ed., s.v. "Edwin Abbott"). The influence of C. H. Hinton on Abbott is given in a long note beginning on page 132, with no reference to the same but longer discussion on pages xix–xxiii.

There is no mention of Samuel Butler's *Erewhon* (1872) among the likely sources of Abbott's 1884 social satire. When Abbott's elevated protagonist asks to descend to Flatland so

as to enlighten its inhabitants, we find nothing on the Eastern religious traditions, then becoming popularized (as in Edwin Arnold's 1879 *The Light of Asia*), about such returns of holy men. There is no note on Abbott's pentagonal house (p. 40) as a likely parody of the faddish octagon houses made famous by Orson Fowler in 1853—nor is the shifted "women's door" into the house commented on (it has moved in the diagram on p. 157 from where it was on p. 40). The mythical Tarnkappe or Tarnhelm (as in R. L. Stevenson's story) isn't cited when an "invisible cap" comes up on page 50, nor is Abbott's renowned brother Evelyn mentioned in the note on the protagonist's "perfectly symmetrical Square" brother (p. 160).

Some of the fun in the text escapes its annotator. Stewart claims (p. 41) that there seems to be only one pun in the book—but in fact puns abound (e.g., on p. 49, how a woman can make herself invisible "ought . . . to be apparent"; and on p. 69, the bridge in "the University of Wentbridge" is "manifestly Cambridge," but the Went—antonym of "came"—is puzzled over in vain).

Were the historical commentary on a par with the mathematical, this would be a fine piece of work. The level of the former, however, is shown in notes such as that on page 77, which quotes without comment a remark that "there was little tolerance for irregularity" in Victorian England (true for much of the middle class, seriously false for the upper and lower classes). Times other than ours deserve a historical imagination informed by thorough scholarship.

ROBERT KAPLAN

Hendrik Antoon Lorentz. *On the Theory of the Reflection and Refraction of Light: Academic Dissertation for Acquiring the Degree of Doctor of Mathematical and Physical Sciences at Leyden University.* Edited and translated by **Nancy J. Nersessian** and **H. Floris Cohen.** xi + vii + 186 pp., frontis., tables. Amsterdam/Atlanta, Ga.: Rodopi, 1997. \$42, £25, Dfl 80 (cloth).

In the 1870s two doctoral dissertations appeared in the sleepy Dutch university town of Leyden that would have a major impact on the development of physics and that would stand at the beginning of the transformation of the Netherlands into what the German physicist Woldemar Voigt later called a "world power in physics." In 1873 Johannes Diderik van der Waals defended his dissertation on the continuity of the gaseous and liquid states; in 1875 Hendrik Antoon Lor-

entz obtained his doctoral degree with a dissertation on the reflection and refraction of light.

Van der Waals's dissertation immediately drew much attention and even inspired James Clerk Maxwell to take up the study of the "low-Dutch language." Lorentz's work—like Van der Waals's, in Dutch—took longer to be noticed, in spite of the publication of an abbreviated German version in the *Zeitschrift für Mathematik und Physik*. But it had a major impact, not only because Lorentz for the first time applied Maxwell's new ideas about the electromagnetic nature of light to phenomena that were previously badly understood, but also because it stood at the beginning of a line of research that would eventually lead to Lorentz's famous theory of electrons. Already in the dissertation we find two major ideas that would guide Lorentz in the further development of his work in electrodynamics: the first is an unwavering belief in the atomistic constitution of matter, the same belief that was central to Van der Waals's work; the second is the conceptually very important and fruitful separation between ether and matter, ether being the seat and the carrier of electromagnetic phenomena, whereas the charged particles that constitute matter are the source of electromagnetic action. A third guiding principle would soon follow, what Lorentz himself called the "hypothesis of a single vibrating particle": the hypothesis that in matter charged particles may be present that oscillate harmonically and in this way interact with the electromagnetic field and with electromagnetic radiation.

Whereas Van der Waals's dissertation was translated into German, English, and French during his lifetime, until 1997 the only full translation of Lorentz's work was a French one in his *Collected Papers*. Now, finally, an English translation is available. It was originally intended as part of a multivolume edition that would supplement the *Collected Papers*, but eventually—and unfortunately—only one other volume appeared. It was clearly the intention of the editors to let Lorentz's work speak for itself: there is no annotation whatsoever and only a very brief historical introduction. That is a pity, given the importance of the dissertation for the development of Lorentz's ideas. On the other hand, it means that the reader is forced to focus on the text itself. A number of things are striking here—in the first place, Lorentz's use of Helmholtz's action-at-a-distance reformulation of Maxwell's electromagnetic theory. Lorentz justifies this choice by claiming that it "gives us the advantage of founding the theory on the most direct interpretation of the facts" (p. 27). Later, when he abandoned

Helmholtz's version in favor of Maxwell's own interpretation, Lorentz admitted that the conceptual difficulties of Maxwell's work had also played a role.

Two other remarkable features of the dissertation are Lorentz's mathematical sophistication and the clarity of his exposition. (For those who know Lorentz's later work, this will not come as a surprise.) In a clear and systematic way the reader is guided through the intricate complex of equations that describe the phenomena of reflection and refraction of light (even including the optical properties of metals). The treatment, though mainly theoretical, does not lose sight of experimental results: wherever possible, Lorentz calculates numerical values that can be compared with experiments.

With this careful translation, Nancy Nersessian and H. Floris Cohen have done a favor to every historian who is interested in the development of electromagnetic theory in the nineteenth century.

A. J. KOX

Susan Solomon. *The Coldest March: Scott's Fatal Antarctic Expedition*. 383 pp., bibl., notes, indexes. New Haven, Conn.: Yale University Press, 2001. \$29.95.

This book is for those who are deeply into Antarctic exploration history. It contains important information on the weather in general, and especially temperature, well presented both in words and in diagrams. These data illuminate earlier discussions of Robert Scott; the author has read original diaries and papers. Susan Solomon's thesis is that those who refer to Scott as a "bumper" are wrong, but she uses that word



Scott's photographer Ponting in 1912 (from Solomon, *The Coldest March*, p. 167).

too many times in trying to support her view. In his book *The Ice* (Iowa, 1986), S. G. Pyne sagely suggested that earlier interpretations of whether Scott was either a hero or a fool changed with the generations and probably reflected the changing attitudes of British self-esteem.

Ernest Shackleton turned back from 88°, 23', and the often-repeated story is that he told his wife a live donkey was better than a dead lion. Shackleton's men called him "The Boss," whereas Scott's men termed him "The Owner." Amundsen demonstrated that Scott did not own the South Pole. In *Scott and Amundsen*, his joint biography of these men, Roland Huntford (Hodder & Stoughton, 1979) noted that whereas Amundsen had ninety miles fewer to travel, Scott followed a known route and had a more gentle climb up the Beardmore Glacier. W. W. Herbert (*The Axel Heiberg Glacier: Following Amundsen's Trail Fifty Years Later* [Norskpolarinstitut Årbok, 1961], pp. 106–126) makes no direct comparison, but he leaves no doubt that Amundsen overcame immense difficulties in ascending an unknown path to the polar plateau.

Solomon has been scrupulously honest and includes items that undermine her own thesis. She discusses the loss of three ponies on sea ice early in the expedition and notes that earlier writers point out that Scott entrusted them to a man who had no experience with either ponies or sea ice. She then recounts the determined effort to rescue them and considers it an act of heroism that has been downplayed by others; it is hard to see how failed actions, no matter how risky, cancel out a fundamental blunder. Still, she does include the point that Scott's second in command, "Uncle Billy" Wilson, strongly advised Scott against trusting the ice and Scott ignored him. Later came Wilson's incredible journey with "Cherry" [Cherry-Gerard] and "Berty" [Bowers] to obtain penguin eggs. He packed six tins of fuel for the trip; the party barely survived. What is new to me is that Scott asked Wilson why he was taking so much fuel. Neither incident demonstrates Scott as a well-organized or a thoughtful leader.

Regardless of what transpired earlier, Scott's actions on the polar plateau were incredible. He was obsessed with beating Shackleton's record of journeying farthest south and emphasized it in his diary when he reached that goal. Before that date, he had the last support party cache their skis and march several days without them. At the last minute he added a fifth man to the polar party and sent back three. It is only through grim determination and strong leadership, combined with the luck of good weather, that these men survived.

Considerable forethought went into preparing rations in four-man bags. Scott's last-minute decision to add another resulted in time and effort lost in repacking, a point not mentioned by Solomon. Again, though she wants to refurbish Scott's reputation, she does include a quotation from Scott's diary about the extra time required to cook for five. She also notes the crowding this change in plans caused in the tent. Later, she discusses the furs and sleeping bags and again honestly points out that Scott did not pay sufficient attention to getting the best of these essential supplies. As an equally critical point, Scott had difficulty with leakage of fuel cans on his 1902 expedition; they still leaked in 1911. Amundsen's fuel did not leak, and cans found half a century later still contained fluid. Solomon's analysis of problems caused by the diet chosen for the man-hauling sledge-trip at that time is good, and her speculations about the last few days of Scott and his men seem to me to be outstanding.

The greatest contribution of this book to further understanding of the events lies in its interpretation of the weather. Once the seasonal patterns start to change, the Antarctic winter gets steadily colder, without any break. Even allowing for that factor, Scott recorded six days of gale-force winds and traveled on none of them, whereas Amundsen had the same kind of weather for fifteen days and traveled on eight.

Perhaps four men might just have been able to make it back from the Pole, but five men did not. The book does not present a persuasive case that Scott has been unfairly treated by earlier writers. What is clear is that the Antarctic is unforgiving, with essentially zero margin for error. Scott cut corners on both his expeditions. Bumbler—maybe not; but gambler—yes.

ELLIS L. YOCHELSON

Bruce Curtis. *The Politics of Population: State Formation, Statistics, and the Census of Canada, 1840–1875.* xii + 385 pp., fig., index. Toronto/Buffalo: University of Toronto Press, 2001. \$60, £40.

This excellent book offers important insights for historians of science. Its author, Bruce Curtis of Carleton University, brings to it a longtime admiration of Philip Corrigan and Derek Sayer's *The Great Arch: English State Formation as Cultural Revolution* (Blackwell, 1985). In applying Corrigan's theoretical framework to document the transition of Canada's federal census to a science-based process that cut across tradi-

tional ideological and cultural lines, Curtis has struck gold.

Curtis highlights the complex international, imperial, national, and local processes by which mid-nineteenth-century Canadian censuses were undertaken, carried out, and interpreted. In his view, “the unusually rich Canadian census archive provides a unique opportunity to investigate census making as an experimental science and to draw the links between knowledge forms, state power, and social imaginaries” (p. 5). This claim for experimental rather than observational social science harbors debatable assumptions that Curtis never addresses; nor does he define the “social imaginaries” that provide his frequent leitmotiv.

The real power of Curtis’s analysis lies in his meticulous demonstration of the utter unreliability of the United Province of Canada’s mid-nineteenth-century censuses in providing anything like the snapshot of society that historians have heretofore sought in them. As “unregulated observational protocols applied to ill-defined objects,” these records are “ill-suited to provide quantitative evidence for examinations of large-scale social patterns. That they are often the best available source,” Curtis chides, “is a thin justification for relying on materials that were generated so haphazardly” (p. 314). In contrast, Curtis upholds the first post-Confederation census in 1871 as “a model of nineteenth-century social scientific observation,” combining “extensive investigation of prior census-making practice” with “heuristic exercises intended to chart the limitations of past efforts, field trials of enumeration schedules, extensive training of officials, and remarkable infrastructural work and formal investments” in which “corrections in the field and after the fact remain visible on the census schedules themselves.” “Nothing of this degree of sophistication,” he adds, took place “elsewhere in Canada in the period—not in the few universities, not in capitalist enterprise, not in the activities of private associations, and not elsewhere in the state system” (pp. 314–315).

Curtis rightly identifies in this evidence “a fascinating instance of the nesting of feudal paternalism in scientific rationalism,” personified in the civil servant responsible for the qualities that differentiated the 1871 census from its predecessors. Canada’s Deputy Minister of Agriculture and Statistics, J.-C. Taché (1820–1894), it turns out, was an ultramontane intellectual who shaped this census as “tributary to a fundamentalist Catholic national-ethnic-linguistic imaginary,” enlisting “state power and rational social-scientific practice against Protestant liberalism

and urban, capitalist luxury and demoralization.” Taché’s census thus served “a larger strategy for constructing a franco-Catholic nationality by establishing a cultural *cordon sanitaire*” for the peasant population inhabiting Quebec’s countryside. “By casting his politics of population in the mould of scientific fact,” Curtis concludes, Taché “ensured that they would continue to bear witness as seemingly neutral numbers” (p. 315).

Although revulsion at the “oppressive project” that “reverberate[s]” in various iterations “through Canadian history of the last two centuries” (p. 315) may well be the fashionable response, it would have helped to inquire more deeply into the historical (and historiographical) implications of Curtis’s valuable find: What larger set(s) of assumptions lent consistency to Taché’s arresting outlook, at least in his own mind? To date we have had only hints to go on that historians have altogether too hastily dismissed Quebec and other cultures as uninterested in modern science; Curtis’s affirmation of a motherlode of astonishing subtleties eliminates every excuse to ignore them.

SUZANNE ZELLER

Michael Sappol. *A Traffic of Dead Bodies: Anatomy and Embodied Social Identity in Nineteenth-Century America*. 430 pp., illus., notes, bibl., index. Princeton, N.J.: Princeton University Press, 2002. \$35 (cloth).

Ruth Richardson was the first to chronicle the modern history of dissection. *Death, Dissection, and the Destitute* (Penguin, 1988), her magisterial account of England’s 1832 Anatomy Act, told the riveting story of how the state decided to allocate unclaimed paupers’ bodies to medical schools for dissection. Richardson’s book was definitive: a work of medical history that was also a densely textured social history of English class formation. *Death, Dissection, and the Destitute* delved into everything from burial customs to utilitarian philosophy in order to show that in procuring a steady source of cadavers, the Anatomy Act effectively criminalized poverty itself. Michael Sappol’s *A Traffic of Dead Bodies: Anatomy and Embodied Social Identity in Nineteenth-Century America* aims to do for American history what Richardson did for English history. Modeled after Richardson’s book, *Traffic* tells how dissection helped consolidate the American medical profession and how it contributed to nineteenth-century American identity formation.

Richardson is a hard act to follow, not least because the story of dissection is much the same

in America as it is in England. As members of one of the first truly international professional communities, American doctors saw the importance of dissection right alongside their English colleagues. In both countries, dissection was, until the late eighteenth century, reserved for criminals who deserved a fate worse than death. In both countries, the formalization of dissection as a principal component of medical training during the late eighteenth and early nineteenth centuries met with strenuous resistance from a public that saw dissection as an unconscionable violation of moral and spiritual law. In both countries, the demand for cadavers combined with the absence of a steady legal supply to create a black market in which graverobbers sold extortionately priced corpses to physicians and schools. In both countries, riots protesting the anatomists' wanton disrespect for the grave were common. American states solved the problem just as England did—by passing laws allocating paupers' bodies to medical schools. Americans responded to these laws much as the English did—by gradually, often grudgingly, assimilating anatomy as an unpleasant but important fact of life.

Written in the long shadow cast by Richardson's pathbreaking work, *A Traffic of Dead Bodies* is a history of American dissection that is also a chronicle of one historian's efforts to differentiate his work from the stunningly written, stunningly similar history of English dissection that precedes it. Sappol both tells a story that hasn't been told and finds repeatedly that this story largely replicates that of another country and another book.

Sappol tries to rescue his project from redundancy by digging deep into its uniquely American aspects. There are detailed accounts of how homeopathic, botanical, and eclectic medicine responded to dissection, and there are whole chapters devoted to such varied material as sensational fiction's fascination with dissection, popular anatomy books, anatomical children's literature, and medical museums. This richly evocative material allows Sappol to differentiate his history from Richardson's: where Richardson focuses on how dissection dehumanized the English poor, Sappol concentrates on how dissection consolidated American middle-class identity.

The trouble is that this framework is itself the stuff of received idea. It would be hard to find a more worn master narrative or to name a nineteenth-century phenomenon that has not been read as the key to middle-class identity formation. The novel has been read that way, as have conduct books, commodities, female bodies,

freak shows, orientalism, discipline, and repression—to name a very few. Sappol's focus on "embodied identity" is the least interesting, least useful aspect of his study.

A Traffic of Dead Bodies is an unwieldy combination of original research and outworn theory. Sappol's remarkable material is consistently distorted by a heavy-handed, often uncritical reliance on a mishmash of trendy concepts and chic vocabulary. Judith Butler, Stephen Greenblatt, Mikhail Bakhtin, Eve Kosofsky Sedgwick, and Antonio Gramsci—among others—are prominent presences in Sappol's book. We hear continuously about "performative" identity, about the "cultural poetics" of anatomy, about dissection as a "carnavalesque" and "homosocial" endeavor, about the single-minded quest of upwardly mobile doctors and lay people for "bourgeois hegemony." Race, class, and gender are likewise dutifully and frequently invoked. Theoretical buzzwords and methodological sound bites clutter Sappol's prose, doing more to obscure the value of his project than to enhance it. The bland, almost reflexive character of the book's widest frame belies the quality and depth of Sappol's research, which deserved more subtle, supple treatment.

ERIN O'CONNOR

Johanna Bleker; Sabine Schleiermacher. *Ärztinnen aus dem Kaiserreich: Lebensläufe einer Generation.* 348 pp., figs., tables, bibls., indexes. Weinheim: Deutscher Studien Verlag, 2000.

In this volume the medical historians Johanna Bleker and Sabine Schleiermacher recount the experiences of a group of women who, they conclude, "each in her own way achieved something extraordinary" (p. 9). This collective biography of the first generation of German women who qualified as physicians traces their careers from the era of the German Empire (1871–1918) until 1945. The documentation comes from a collection of materials held by the Institute for the History of Medicine at the Free University of Berlin. The book consists of two sections: the first is a narrative history of both the group as a whole, which consisted of 792 physicians, and some of its individual members; the second is a compilation of statistical data on family backgrounds, regional distribution, academic achievements, and medical specialties. A very useful appendix gives biographical data on those members of the group on which such data exists.

This careful research challenges many accepted views of this generation of German aca-

demic women. Contrary to the assumption that female physicians came from a higher social class than their male counterparts, this data suggests that their backgrounds were similar; many came from small towns or rural environments. One conventional view that the data confirms is the large number of Jewish women who studied medicine—by some estimates, about 30 percent of female medical students were Jewish. But many of these came from impoverished small-town backgrounds rather than from the wealthy and educated households that were well known for their support of their daughters' educational ambitions.

The women's careers were shaped, as one might expect, by gender stereotyping and discrimination. Their most common medical specialty was pediatrics, a field that was considered particularly suitable to women and was not very attractive to ambitious men. Contrary to the usual pattern among professional women of this era, over half of these physicians married, and many had children. And despite ever-present discrimination, most succeeded in supporting themselves financially through private medical practices. Some made notable contributions to medical research, public health, or politics.

The final chapters of the study recount the fate of these women after the Nazi takeover in 1933. The group was suddenly divided into two segments: those whose careers came to an abrupt and catastrophic end and those for whom normal life continued for a few years longer. The account of the difficulties of those who emigrated to the United States and struggled to rebuild their careers there is particularly well documented and vivid.

The story of those who remained in Germany, though as a whole fascinating, distorts the historical record in one important way. Many of this group, the authors tell us, perceived a substantial continuity between the theories of eugenics and racial hygiene that they had held throughout their careers and the policies of the Nazis and thus adapted easily to the new regime. Such ideas are attributed only to those physicians who held what the authors call "national-conservative or *völkisch*"—that is, right-wing—political views. But eugenics and race hygiene found just as many, perhaps even more, adherents among physicians on the left and were probably espoused by many of those who went into exile as well as those who remained. For example, Anna Margaretha Stegmann, who is included in this sample, was a socialist physician and Reichstag deputy who was also an advocate of eugenic legislation during the Weimar period (see Mi-

chael Schwarz, *Sozialistische Eugenik: Eugenische Technologien in Debatten und Politik der deutschen Sozialdemokratie* [Dietz, 1995], pp. 150, 248). To associate eugenics only with right-wing or National Socialist political beliefs is to simplify its complex history.

Ärztinnen aus dem Kaiserreich adds an important dimension to the history of women in science and medicine. The entry of women into these fields is often portrayed as a feminist cause, political campaign, or administrative problem. Apart from a few well-known figures, the individuals who accepted the difficult role of pioneer often remain unknown and the effects of the struggle on their lives unexplored. This is a work based on massive and careful research that also tells a moving story.

ANN T. ALLEN

Kathryn A. Neeley. *Mary Somerville: Science, Illumination, and the Female Mind.* (Science Biographies.) xvi + 256 pp., bibl., index. New York: Cambridge University Press, 2001. \$65 (cloth).

In 1827 the prominent politician and educationist Lord Henry Brougham wrote to Mary Somerville, asking her to translate Pierre Simon Laplace's five-volume epic *Mécanique céleste* (1799–1825). With the advice of the astronomer John Herschel and the support of her husband William, Somerville published *Mechanism of the Heavens* in 1831. Although the task of making comprehensible Laplace's famously opaque work was one she undertook with some trepidation, the result was acclaimed by her contemporaries in the elite of British science. The extensive preface to this book was subsequently published as a "Preliminary Dissertation" (1832), and it in turn provided the basic structure for *On the Connexion of the Physical Sciences* (1834), which was widely read. Some of the major themes in Somerville's long career are evident in this brief period: her friendship with and mutual respect for the leading figures of early nineteenth-century British science; her appeal to a range of audiences; and her ability not only to comprehend Laplace, but to present his ideas in a lucid form. In Kathryn Neeley's study we encounter Somerville as a poet-mathematician-astronomer, a science "illuminator" who was "skillful in recreating the experience of the scientific sublime for her readers" (p. 9), publishing a series of books assessing the cosmos in its entirety, from state-of-the-art physics to *Physical Geography* (1848) and *Molecular and Microscopic Science* (1869). Neeley is respectful of

Somerville's skill, both as a writer and as a woman who delicately navigated the sensibilities of her bourgeois audience and the norms of a male-dominated, increasingly professional scientific community.

Historians of science have found it difficult to categorize Mary Somerville. She did not leave us with any of the conventional yardsticks by which the conventional great man of science was measured: a discovery, a unit, or a technique. Should she instead be cast as a "popularizer"? This does not sit well either—even though recent scholarship has removed the derogatory overtones of the term, it still fails to describe Somerville adequately and is in any case arguably anachronistic for much of her active life. Neeley tackles this question head-on—this is not a conventional biography in structure or content. Rather, it is an analysis of the commemoration (or "forgetting") of Somerville surrounding a detailed exegesis of her published works. The former encompasses the responses she elicited, including the founding of Somerville College, Oxford, and her own *Personal Recollections* (1873). In the latter Neeley is adept at combining literary analysis with a presentation of the technical material. She pays careful attention to the genres and narrative themes of Somerville's works: her blend of nature, science, and natural theology; "the role of the feminine in her writing," if any (p. 123); and her reframing of Laplace's mathematics within the belief systems of the rising middle classes.

Neeley thereby contributes to the recent wealth of work on science writing, an integration of gender studies and science-in-culture. She places Somerville in context as a nineteenth-century woman negotiating a position at a time when the boundaries of gender were undergoing conflict and change. It would be interesting to expand on the brief comparisons with other visible women—George Eliot or Harriet Martineau. Neeley's historiographical analysis of the changing strategies and roles for women in science over the course of Somerville's long life (1780–1872) deserves to be read by any serious scholar of nineteenth-century science or women's history.

JOHANNA ALBERTI
SAMUEL J. M. M. ALBERTI

Margaret A. M. Murray. *Women Becoming Mathematicians: Creating a Professional Identity in Post-World War II America.* xviii + 277 pp., illus., tables, apps., bibl., index. Cambridge, Mass./London: MIT Press, 2000. \$29.95.

In *Women Becoming Mathematicians*, Margaret Murray pioneers a new form of writing, inter-

weaving the biographies of thirty-six women with doctorates in mathematics to address significant questions. How do successful women feel about their careers? Their marriages? Their teachers? The sexism that they faced? What sexism *did* they face—that they recognized and can remember? How did they respond to it at the time? How do they think in retrospect that they should have responded? What do they consider success? How well have they measured up to their own standards? Are there differences between their career patterns and the prevailing myth describing the life of a successful mathematician?

Some answers emerge. Mathematicians, according to the myth, are devoted to mathematics as strange children and are obsessed by it throughout the days and hours of their lives. Murray proves that women mathematicians do not follow the pattern of the myth. She leaves open the question of whether this myth describes the lives of most male mathematicians—or just the very few in the history books.

Women Becoming Mathematicians considers the approximately two hundred women who earned Ph.D.'s in mathematics in the United States during the 1940s and 1950s but focuses primarily on the thirty-six who granted the author extensive interviews. All were children with wide-ranging interests, and most didn't choose mathematics until college. One ended up in college administration and several wandered into nonacademic careers in applied mathematics, but most became tenured college or university faculty. Two of the thirty-six interviewees were the second women presidents, respectively, of the American Mathematical Society and the Mathematical Association of America; the first women presidents of each had died by the time Murray's research began. All faced significant sexism, but their responses varied widely.

Exactly half became mothers, several of four or five children. Mary Ellen Rudin, a successful research mathematician, describes doing mathematics with her four children playing around her. She claims that most mathematical research is done in spurts, "not with continuous thought" (p. 188). Others report needing concentrated time.

This is a book about trends, so the stories of the individual women are intertwined. There is a fine index, however, so the biography of each subject can be reconstructed if a reader wants to make the effort. There are a few mistakes. Murray repeats the falsehood I have been perpetrating for twenty years: that Evelyn Boyd Granville was the first black woman to earn a doctorate in

mathematics; it wasn't until 2001 that Johnny Houston and Scott Williams independently realized that Euphemia Lofton Haynes was a black woman who did so earlier in 1943. Murray says that Charlotte Angas Scott "earned" her Sc.D. "at" the University of London in 1885, when really Scott studied only at Cambridge University with Arthur Cayley; her degree is from the University of London by a special arrangement because Cambridge did not grant degrees to women for another six decades. Murray's chart on page 5 showing the total number of doctorates granted in mathematics in the United States every five years is not wrong, but it is misleading: many more men than women came from other countries to pursue graduate work in mathematics in the United States; thus the percentage of women is artificially low. Actually, toward the end of the twentieth century about a quarter of the Americans earning doctorates in mathematics were women.

These are minor matters. This is a superb, fascinating book that probes both the insecurities and the achievements of thirty-six remarkable women. It also explores how mathematicians in particular and successful people more generally emerge from a social context and through a series of personal decisions.

PATRICIA CLARK KENSCHAF

P. G. Lejeune Dirichlet. *Lectures on Number Theory*. Supplements by **R. Dedekind**. Translated by **John Stillwell**. (History of Mathematics: Sources, 16.) xx + 275 pp., apps., index. Providence, R.I.: American Mathematical Society, 1999. \$49 (paper).

Based on Peter Gustav Lejeune Dirichlet's lectures at the University of Göttingen and published posthumously, the *Vorlesungen über Zahlentheorie*, here translated into English, has at least two claims to fame: one is due to its author, the other to its editor. In his courses in Berlin and Göttingen, Dirichlet (1805–1859) presented in a concrete and accessible fashion the most important ideas and results of that bible of nineteenth-century number theory, the 1801 *Disquisitiones arithmeticae* of Carl Friedrich Gauss, starting with elementary results on divisibility and congruence and advancing to its core, the classification of quadratic forms. If only for this reason, these lectures have played a decisive role in shaping the development of the subject. But their editor, Richard Dedekind (1831–1916), also added crucial supplements. Those of the first edition, in 1863, focused on Dirichlet's own

achievements, in particular his spectacular use of analytical methods to prove number-theoretical results—for instance, that any arithmetical progression (with relatively prime initial term and difference) contains infinitely many primes. But from the second edition of 1871 on, Dedekind's last supplements elaborated his own contributions to the topic, offering a set-theoretical point of view and a *cortège* of new notions (for instance, that of a "field" or of an "ideal") that would contribute decisively, a few decades later, to the structural approach characterizing a large part of "modern mathematics."

The edition translated here, though not explicitly indicated, is mainly the fourth, of 1894 (for instance, the criterion characterizing quadratic residue is identified in the text as Euler's [p. 53], an attribution that was not mentioned at this point in the 1863 edition). However, John Stillwell, the translator, has omitted the last two supplements of this edition, which will be disappointing to anyone interested in the subtle evolution of arithmetic during the second half of the century and in the bridge that was slowly built, through the successive editions, between Gauss's classical synthesis and modern algebraic number theory. The sheer bulk of these last supplements (more than a hundred pages) makes the decision understandable. The suppression of Dedekind's voice, however, extends even to his prefaces; this choice is much less understandable, as these prefaces carefully explain the various sources used for the text and thus, paradoxically, would have helped the reader to identify Dirichlet's genuine writing amid Dedekind's editorial efforts. Instead, Stillwell provides his own introduction, which usefully summarizes, in chronological order, the main results known before Dirichlet but does not discuss Dirichlet's own background, the context of the publication of the book, or the deep conceptual transformations of number theory since Euclid's *Elements* (the point of departure chosen in the introduction).

The translation and the mathematical commentaries are competent (although the analogy between Andrew Wiles and Carl Gustav Jacobi on the computation of a class-number formula [p. xviii] is misleading: Wiles did not find the missing formula within a month; rather, he contrived to avoid the need for it!). As it stands, then, this translation cannot serve as a source in the historical sense; nonetheless, it should be a source of learning and enjoyment for the audience it is clearly aimed at—mathematicians, present and future.

CATHERINE GOLDSTEIN

Armand Borel. *Essays in the History of Lie Groups and Algebraic Groups.* (History of Mathematics, 21.) xiii + 184 pp., bibl., indexes. Providence, R.I.: American Mathematical Society; London: London Mathematical Society, 2001.

In this collection of essays, four of which are slight modifications of papers that appeared earlier, Armand Borel examines the details of important aspects of the theory of Lie groups and algebraic groups. An introductory chapter briefly summarizes the foundational ideas of the Norwegian mathematician Sophus Lie, as well as those of mathematicians who pursued the questions raised by Lie—Wilhelm Killing, Élie Cartan, and Hermann Weyl among them. The next chapter presents the details of a number of proofs from the turn of the twentieth century of a particular result in Lie group theory, the complete reducibility theorem for $SL(2, \mathbb{C})$. Chapters 3 and 4 consider the contributions of Weyl and Cartan, respectively. The remaining four chapters focus on algebraic groups, first chronologically, with chapters on nineteenth- and twentieth-century developments, and then by examining the contributions of Claude Chevalley and of Ellis Kolchin.

Born in Switzerland, receiving his Ph.D. in mathematics from the University of Paris in 1952, Armand Borel began his distinguished career at the Institute for Advanced Study's School of Mathematics in 1957. He worked for twenty years as a member of Bourbaki, the collective name for an influential group of mathematicians who sought, beginning in the 1930s, to reformulate all of mathematics into a single axiomatic system. As an important contributor to research in the theory of Lie groups and algebraic groups, Borel is eminently qualified to explicate and synthesize the writings of the central contributors to the emergence and development of these specialized fields.

It is generally acknowledged that specialization in mathematics over the last century has made communication across subdisciplines at more than a very general level challenging at best. The difficulties facing the historian who wishes to analyze mathematical developments of the last hundred years loom even larger, as specialized training in a particular discipline of mathematics is difficult to combine with historical training. Thus the mathematician who seeks to bring to light details of the history of his subject has the opportunity to offer a great service to fellow mathematicians as well as to historians of science.

Such mathematicians are sometimes unaware of the questions that interest historians. They are often accused of writing about the history of their subject in order to uncover the steps that led to the current version of a particular theory, neglecting to consider why those steps followed certain paths and not others. Because they usually have other mathematicians for their audience, they typically focus on the internal aspects of a theory's history—who proved what theorems and how—paying less attention to the broader cultural and historical context in which their subjects worked. Finally, mathematicians writing history may blur (if not entirely ignore) the distinction between modern notation and definitions and those of the researchers they study.

The present work, written by a mathematician for mathematicians, exhibits a number of these traits. From the start, the author acknowledges that he will mostly use present-day notation, and he occasionally alerts the reader to the important differences between a modern formulation of a particular concept and how his subjects would have understood it. But these differences are not Borel's main concern, and not many readers will finish this book with a clear sense of how the questions and ideas he treats changed over the century under consideration. While Borel occasionally quotes from correspondence and reminiscences of the mathematicians he writes about, he draws primarily from their published papers and monographs, providing no information about the role played in their research by their educational backgrounds, their professional responsibilities, or their personal connections with other mathematicians. Finally, although the author makes a number of observations that raise questions about the paths taken (or not) by researchers in the fields of Lie groups and algebraic groups, he makes no substantial attempt to answer those questions.

This book's avoidance of important historical questions would alone make it a frustrating read for most historians of science. Nearly all of them will find that the high level of knowledge of Lie groups assumed by the author makes it impenetrable. Specialists in the field may enjoy the opportunity to examine their heritage from a technical point of view, but this book will leave them with only a limited understanding of the historical development of their subject.

PATTI W. HUNTER

Xiang Chen. *Instrumental Traditions and Theories of Light: The Uses of Instruments in the Optical Revolution.* (Science and Technology Series.) xxiii + 211 pp., figs., illus., notes, bibl.,

index. Dordrecht: Kluwer Academic Publishers, 2000. \$99 (cloth).

History of science once concentrated on the theoretical side of basic physical science, and recent trends have emphasized the social influences and interactions more fully. *Instrumental Traditions and Theories of Light* represents another approach, one that looks at the practices of the experimental side of science and its influence on concepts and theories. Although Thomas Young introduced wave theory to British science at the beginning of the nineteenth century, the still-strong influence of Newton's particle optics in the form of the ray theory of light prevented its easy acceptance. Neither theory was superior in explanatory power. Moreover, the proponents of each were not, to some extent, speaking the same language. As Jed Buchwald put it in *The Rise of the Wave Theory of Light* (Chicago, 1989), to which this work is a valuable supplement, "Experiment constrains and is constrained and motivated by theory; theory constructs the world by specifying what is accessible to experiment. Consequently, to concentrate one's history on theory not only misses half the story but inevitably obscures what the theory was about" (p. xxiii). That other half of the story is effectively pursued by Xiang Chen through his analysis of the two "instrumental traditions" that dominated nineteenth-century optics.

In his second chapter Chen shows how experiments were suggested by the wave or ray tradition and the way in which this led to different systems of classification, "taxonomies," in optics. One crucial problem for the ray theory was the handling of diffraction and polarization. Three taxonomies were proposed in the early nineteenth century to support the ray theory, the wave theory, and a combination. The result was confusion. Since these taxonomies reflected the systems of categories used by the protagonists in the debate of wave versus ray, their change over time reflected the profession's shift to the wave theory by midcentury.

In the following chapters Chen describes in some detail instrumental practices in the areas of dispersion, polarization, and intensity of light that came to define "instrumental traditions" within optics. Another topic is the growing disparity between the geometrical theories of the ray camp and the algebraic theories of the wave theorists. Two instrumental traditions emerged, the visual and the geometric. In the older visual tradition, instruments were developed as aids to the eye, which was used as a measuring device. In order to protect the eye's sensitivity, compro-

mises were made in procedure; but these led to serious discrepancies in the results, rendering resolution of the disputes between wave and ray proponents impossible. Other scientists, who placed less confidence in the eye as an optical instrument, devised instruments that minimized the role of the eye, instruments that different observers could use to obtain the same results. To practitioners of the growing specialty of mathematical physics, this geometrical tradition was particularly acceptable since it supplied the kind of numerical data they needed. Thus the wave theory became the accepted theory of physical optics while the ray theory lingered on in the production of optical devices for popular use. Chen is careful to conclude that this is not a Kuhnian revolution but a more subtle rivalry between instrumental traditions that were never articulated in a theoretical paradigm.

Were there no problems with this work other than occasional redundant paragraphs, one could pass them off as casualties of long-distance editing. However, many of the illustrations fail to contribute to the work. They are imprecisely drawn, often confusing, and sometimes quite unphysical, and they do not always reflect the textual descriptions next to them. These shortcomings notwithstanding, this book is particularly valuable for its clear analysis of the role of instrumental/experimental practices in the history of science.

JOHN L. MCKNIGHT

Michael Schaaf. *Heisenberg, Hitler und die Bombe: Gespräche mit Zeitzeugen*. 148 pp., frontis., illus., app. Berlin/Diepholz: GNT-Verlag, 2001. €22.50 (paper).

Heisenberg, Hitler und die Bombe is, thankfully, more than another attempt to cash in on the public fascination with the nonexistent German atomic bomb. Through interviews with six former German fission researchers and two former colleagues of Werner Heisenberg—Edward Teller and Friedrich Hund—Michael Schaaf delves into such issues as the ideological background to the project, the reasons for its failure, the relationships with Russian scientists, the scientists' motivations for working on the project, and their ethical scruples, if any, regarding such work.

In addition to Teller and Hund, the interviewees include Carl Friedrich von Weizsäcker, Manfred von Ardenne, Willibald Jentschke, Rudolf Fleischmann, Erich Bagge, and Paul Harteck. All of the interviews took place in the middle 1990s, except for the one with Harteck, con-

ducted by Heinrich Medicus in 1982. All of the subjects were well advanced in age, and four have since died. Several had not been interviewed previously for public use. It is unfortunate that more of the lesser-known figures were not included. It is also regrettable that Schaaf apparently did not consult any of the previously available interviews of these men; and it is even more regrettable that he did not incorporate any of the extensive historical literature on German fission research. The lack of an index and a bibliography further weaken the book.

All of Schaaf's subjects were still remarkably sharp, but his attempts to rekindle their memories of details with copies of documents naturally did not succeed. For people of this age, it is their surviving memories of motivations and relationships that are perhaps the most valuable. Such memories, if still vivid, were probably intensely felt at the time and likely to be revealed now without reservation. Schaaf's subjects do not disappoint. In so doing, they provide brief insights into how Hitler succeeded and fission research failed.

Several of the interviewees admitted that they worked on fission only for the funding it provided and with no concern for its wider implications. Some still portrayed themselves as opponents or victims of the regime. When asked if he had noticed any impact of the persecution of Jews, Fleischmann said: "Politics did not interest us. We were concerned with physics things" (p. 75). Von Weizsäcker revealed how he convinced Heisenberg and Otto Hahn to join the research effort by playing upon their personal ambitions.

The widely differing perceptions of Heisenberg, the leading fission scientist, indicate how infighting helped doom the project. Only Bagge and von Weizsäcker had worked directly with Heisenberg on fission. Nearly all, including Bagge, blamed Heisenberg and von Weizsäcker, whom Bagge considered a rival, for the failure to obtain even a working reactor. Von Ardenne blamed them for fatal errors. Harteck was still bitter about the treatment he received from Heisenberg and his circle: "these physicists who have no feeling for the material and are filled with self-importance!" (p. 108).

On the other side, Teller (a member of the Manhattan Project) defended Heisenberg by repeating the now largely refuted thesis that the German bomb project failed because Heisenberg deliberately sabotaged it. This, he claimed, is proven by Heisenberg's later misunderstanding of how a bomb works. When shown Teller's comment, von Weizsäcker would agree only

about Heisenberg's lack of understanding, not about the claimed sabotage.

Such remarks would have been of even more historical value if Schaaf had incorporated into these interviews some of the results of recent historical research as well as other available interviews of these and other German fission researchers.

DAVID C. CASSIDY

Sverre Petterssen. *Weathering the Storm: Sverre Petterssen, the D-Day Forecast, and the Rise of Modern Meteorology.* Edited by **James Rodger Fleming.** (Historical Monograph Series.) xiv + 329 pp., frontis., apps., index. Boston, Mass.: American Meteorological Society, 2001.

Sverre Petterssen (1898–1974) is a name known to all who studied meteorology in the mid-twentieth century. A Norwegian who helped introduce the new Bergen-based polar front school of forecasting to the United States in the 1920s, he became one of the world's leading authorities on the analysis and prediction of weather. The numerous editions of his textbooks, beginning in 1940, helped the American meteorological profession to consolidate its scientific claims and to train large numbers of new practitioners. Petterssen contributed to establishing meteorology as an academic subject while heading departments at MIT and the University of Chicago, served as scientific director of the U.S. Air Force's Weather Service, discovered what was later called "the jet stream," and helped vitalize postwar international cooperation. He also played a critical but at times contested role in the forecasting effort for the D-Day landing on Normandy. Before dying, Petterssen wrote an autobiography in English that was translated and published in Norwegian (*Kuling fra nord: En værværslers erindringer* [Aschehoug, 1974]). With the support of the American Meteorological Society's highly valuable program for publishing historical monographs, James Fleming obtained a copy of the original English manuscript and edited it to produce the present volume.

Petterssen's narrative provides many valuable glimpses into and commentaries on the workings of meteorological institutions before, during, and after World War II. This was a period in which the Bergen methods, based on the frontal-cyclone model, spread beyond Norway, often on the wings of commercial and military aviation's challenging meteorological needs. Although the Bergen school's initial breakthroughs after World War I provided techniques for analyzing

and predicting weather using three-dimensional models, it had to rely largely on observations from the earth's surface. It was not until the late 1930s that new technologies allowed regular and extensive observations from aloft, which in turn opened the way for new theoretical understandings and forecasting methods. Just as World War I prompted major changes in meteorology, World War II, the Korean War, and the Cold War served as motors for accelerating scientific and professional change. Petterssen's career and activities touch cogently on these developments.

Petterssen's story is self-centered and gossipy; still, many of the details will provide historians with insights that cannot readily be gleaned from archival sources. He offers occasional digressions into the different methods of forecasting; these can give the nonmeteorologist an adequate sense of what was at stake among the competing schools. His accounts of actual practice reveal the dilemma of weather forecasting just as computerized numerical forecasting was beginning to be developed. Although committed to creating methods based on physical theory that aimed to eliminate the subjective human factor in forecasting, he shows how an intuitive feel for the weather and an ability to intuit physical processes where direct observations were lacking contributed to his own successes. Petterssen's detailed accounts of the organization and workings of the Allied forecasting services for Europe and of the high drama of the D-Day forecast will have to be consulted by anybody attempting a more systematic study of this chapter in military and science history.

But Petterssen reveals more than he perhaps intended. He tries much too hard to impress and too often seems obsessed with having been cheated out of sufficient credit for his achievements. Coming from a tiny village on the north Norwegian coast, Petterssen was a self-made man who perhaps never felt fully at ease with what he accomplished. Whatever the reason, his narrative is at times pompous and his memory highly selective, to the extent that the reader must wonder why certain historically important Mr. X's and Y's remain anonymous, while names and details of incidental characters are served up for little apparent reason. Had Petterssen not been so preoccupied with himself, his autobiography could have been a considerably greater source of insight into this rich chapter in the history of meteorology. What he does provide is cultural history, via a narrative self-construction that reveals the life and mind-set of one of several Norwegians who found their way, in the earlier part of the twentieth century, from

rural poverty and provincialism into the world of international science.

ROBERT MARC FRIEDMAN

Tom Rea. *Bone Wars: The Excavation and Celebrity of Andrew Carnegie's Dinosaur*. 276 pp., illus., bibl., index. Pittsburgh: University of Pittsburgh Press, 2001. \$25 (cloth).

Andrew Carnegie's dinosaur was a composite specimen of *Diplodocus* found near Sheep Creek, Wyoming, in July 1899 by collectors in his employ. Though not so massive as the better-known *Brontosaurus* (today renamed *Apatosaurus* or possibly *Atlantosaurus*), *Diplodocus* had one outstanding characteristic: at 130 feet, it was the longest dinosaur yet found. Carnegie, who had risen from humble beginnings to become one of the richest men in the world, took full advantage of its spectacular size by having casts made of its skeleton. He then sent full sets of these casts to museums around the world.

As *Diplodocus carnegii*, a designation that still stands, plaster models were erected bone-by-bone in the new Dinosaur Hall in the Carnegie Museum of Natural History in Pittsburgh; at the British Museum (Natural History) in South Kensington, London; at national museums in Berlin, Paris, Vienna, and Bologna; at the Imperial Academy of Sciences in St. Petersburg (and now in Moscow); at national museums in Argentina, Spain, and Mexico; and indoors or outdoors in cement or fiberglass in several American resurrections since, most recently at the Carnegie Museum of Natural History in Pittsburgh (1999). All but the latest few of these were gifts by Carnegie to the institutions involved. Because of his generosity, persons throughout the world could see for themselves just how big the dinosaurs had been.

Diplodocus is a sauropod, and the first significant remains of that group (*Cetiosaurus*, *Pelorosaurus*) were discovered in early nineteenth-century England. *Diplodocus* itself (the genus) was discovered and named by O. C. Marsh in 1878. He established the dinosaur suborder *Sauropoda* the same year. Except for its impressive size, therefore, the discovery of *D. carnegii* in 1899 was not a scientific breakthrough. But when curators at Carnegie's museum attempted to complete and mount the skeleton they had found, all sorts of new questions arose as to the proper angle of the head with regard to the neck, the assumed gait and placement of the limbs, and the manner in which the long tail was dragged or carried. It had often been thought that the heavier sauropods like *Apatosaurus* were nec-

essarily confined to water in order to support their huge bodies. With the long but comparatively slender *Diplodocus*, that expedient seemed less necessary. Mounting the skeleton forced dinosaur experts to interpret *Diplodocus* as they thought it must have been in real life.

Stimulated by the discovery of *D. carnegii* and the attendant publicity, several American institutions in eastern states sent collecting expeditions west to the Wyoming fossil beds. A number of important papers were written about sauropods as a result, and in 1909 the sauropod-rich quarry at what is now Dinosaur National Monument was discovered. The *Diplodocus* find at Sheep Creek in 1899 was therefore of first-rate importance as a cultural and scientific motivator.

Tom Rea, a freelance journalist who writes exceptionally well, has close ties to both Pittsburgh and Wyoming. Utilizing local knowledge and considerable research, he has crafted a fine brief history of important episodes in the history of dinosaur discoveries by Americans between the Cope-Marsh feud and the Asian adventures of Roy Chapman Andrews. The inclusion of numerous period photographs is a strong point, and there is a map on page 100. Virtually all of Rea's few slips pertain to antecedent British topics. Thus, *Iguanodon* is mistakenly called "duck-billed" (p. 161), and it was not first discovered in 1878; Richard Owen (1842) failed to identify *Cetiosaurus* as a dinosaur (p. 180); and Archibald Geikie's name is misspelled throughout (pp. 10, 246, 273). On the American side, important papers by Marsh and others are surprisingly absent. Even so, Rea's book is informative, very readable, well designed, and reasonably priced. I recommend it to all those interested in dinosaurs.

DENNIS R. DEAN

Christoph Irmscher. *The Poetics of Natural History: From John Bartram to William James.* xx + 354 pp., illus., bibl., index. New Brunswick, N.J./London: Rutgers University Press, 1999. \$42 (cloth).

This is a handsome book with a beautiful dust jacket, featuring a painting of snowy owls by John James Audubon, and sixty-four text illustrations. The book opens with a view of the long-term friendship and frequent botanical exchanges, entirely dependent on the vagaries of the post, between two Quaker plant enthusiasts, John Bartram in Kingsessing, Pennsylvania, and Peter Collinson in England. Excerpts from their correspondence are generously sprinkled through

the narrative, helping one to grasp the nature of their relationship. Further along in the first chapter, Christoph Irmscher explores the world of William Bartram (John's son), examining in some detail his book on travels in what would become the southeastern United States.

Chapters 2 and 3 investigate Charles Willson Peale and Phineas Taylor Barnum, Peale with his Philadelphia Museum and Barnum with his American Museum in New York City, both of which provided visitors with entertainment and perhaps a little education along the way. The author moves seamlessly from analyses of Peale's paintings *Noah and His Ark* and *The Artist in His Museum* to General Tom Thumb, the Feejee Mermaid, Commodore Nutt, and other denizens of the bizarre universe of P. T. Barnum. "The Power of Fascination" (Ch. 4) features rattlesnakes and some (e.g., Cotton Mather, Jonathan Edwards, Mark Catesby, William Bartram, and John Edwards Holbrook) who were bedeviled or captivated by those marvelous animals. Holbrook's monumental contribution to natural history, *North American Herpetology* (facsimile reprint ed.; Society for the Study of Amphibians and Reptiles, 1976), is explored, and four illustrations of rattlesnakes from this volume and a number from other sources grace the pages of *The Poetics of Natural History*.

In Chapter 5, "Audubon at Large," Irmscher displays a fascination with woodpeckers. Illustrations of those engaging birds from the works of Catesby, Alexander Wilson, and Audubon appear, along with a number of other renderings of birds by Audubon. Unfortunately, none are in color. Quotations from Audubon support the author's evaluation that "*The Birds of America* marked the pinnacle of the 'poetics of natural history'" (p. 234). In the closing chapter, "Agassiz Agonistes," the cosmos of Louis Agassiz is examined—including his collecting trips to Lake Superior and the Amazon (the Thayer Expedition). Agassiz's unrelenting opposition to Darwinian theory and miscegenation receive substantial attention, as does the view of his student William James toward the Thayer Expedition. Agassiz was a genius who contributed enormously to the understanding of the world's biota, but by the time of the Thayer Expedition (1865–1866) he was well past his zenith, a fact that seems to have escaped him.

It is easy to point out, as Irmscher does, the shortsightedness of our eighteenth- and nineteenth-century predecessors, many of whom wantonly destroyed wildlife; but contemporary treatment of the natural environment and its inhabitants will not be judged gently by critics of

the future. Although armed with vastly superior knowledge and incomparably better means of communication, we as a species continue to treat this planet as if it were our playground to abuse at our leisure and pleasure—ignoring long-term effects of global warming, damage to the ozone layer, improper storage of radioactive and toxic wastes, and destruction of natural resources.

The Poetics of Natural History is a pleasure to read. It is hard not to like a book with numerous illustrations of and anecdotes on rattlesnakes, to say nothing of tidbits on buzzards, consideration of dog piss weed (common toadflax), tipitiwitchet (Venus flytrap), and Constantine Samuel Rafinesque, and passing mention of swallowwort, skunkweed, and Jo-Jo the Dog-faced Boy. Although well chosen, many of the illustrations are too dark, and a number are too small.

Irmscher took a unique approach to writing about natural history, stating that his purpose was “to help dispel the still widespread assumption . . . that only ‘belles lettres’ reveals to us our presence as desiring, storytelling beings within the concrete world of experience” (p. 9). In my view, he did what he set out to do, and he did it in grand style.

WILLIAM D. ANDERSON, JR.

Robert Paddle. *The Last Tasmanian Tiger: The History and Extinction of the Thylacine*. x + 273 pp., illus., figs., tables, bibl., index. Cambridge/New York: Cambridge University Press, 2000. \$64.95.

This is a timely book. Over the last few months plans to revive the so-called Tasmanian tiger, extinct since 1936, through cloning have been aired on television and have made headlines in the press. These plans would appear to be rather far-fetched, but they seem to have succeeded in putting the Tasmanian tiger on the map.

The Tasmanian tiger, also called the marsupial wolf, was neither a real tiger (*Panthera tigris*) nor a real wolf (*Canis lupus*). Nowadays, scholars who want to avoid confusion use the term “thylacine,” and the scientific name is *Thylacinus cynocephalus*. Even before its recent prominence the animal had earned itself a certain notoriety, as decades after its extinction—the last specimen died in captivity in the Hobart Zoo on 7 September 1936—sightings of the Tasmanian tiger were still being reported. Apparently the thylacine readily attracts myths and legends. Robert Paddle’s recent book on this animal illustrates this point abundantly.

The subtitle of Paddle’s book—“The History

and Extinction of the Thylacine”—is somewhat inaccurate. The book is really, as the author acknowledges on page 1, a history of the “changing scientific perceptions of the thylacine’s predatory behaviour.” It is a detailed account of the “social construction of scientific knowledge” regarding the Tasmanian tiger (p. 235) and, therefore, an example of what we usually call history of science. A “history of the thylacine” is something else, although it would doubtless include many of the data gathered by Paddle.

The first scientific description of the animal dates from around 1800. At that time the animal was extinct or almost extinct in mainland Australia and no longer to be found outside the island of Tasmania. It has been often assumed that the thylacine had lost out on the mainland to the more recently arrived dingo, but according to Paddle hunting by groups of Aborigines may have played a role as well.

A large part of the book is dedicated to an analysis of contemporary (mainly nineteenth century) reports on the thylacine’s predatory behavior, with emphasis on its alleged propensity to large-scale sheep killing. The author demonstrates convincingly that although Tasmanian tigers did occasionally kill sheep, this reputation was a gross exaggeration. He argues that the thylacines functioned as scapegoats when, during the early nineteenth century, flocks of sheep were faring badly for unrelated reasons. A bounty scheme was introduced in order to rid the sheep-keeping areas of this alleged menace, but few bounties were claimed, suggesting that the thylacine cannot have been much of a threat. When much later in the century (1888) a new bounty scheme was introduced over a much wider area, the response was better, and in 1909, when the scheme was terminated, claims had been made for 2,207 specimens (p. 167).

It was a blow from which the thylacine would not recover, even though around the same time the notion that the animal was on the brink of extinction and ought to be protected was gaining ground. However, opposition to such a measure was still so strong that the Tasmanian tiger was not granted protected status until 1 July 1936, sixty-nine days before the last specimen died!

So far the story is rather straightforward, based on solid research and convincingly argued. Personally, I am less impressed by what the author has to say about the perceptions and reactions of the scholarly community. He argues that naturalists were so much influenced by the paradigm of progressive evolution that they were not particularly interested in saving a marsupial animal such as the Tasmanian tiger; marsupials

were supposed to give way to placental animals anyway. He calls this “placental chauvinism.” Nor am I impressed by the facile moralistic tone of the book or by the sledgehammer lack of subtlety with which Paddle conveys his message. However, the reader who is willing and able to curb his or her irritation at these flaws will have an interesting read.

PETER BOOMGAARD

The Scopes Trial: A Photographic History. Introduction by **Edward Caudill**. Photo captions by **Edward Larson**. Afterword by **Jesse Fox Mayshark**. [xvi] + 88 pp., illus. Knoxville: University of Tennessee Press, 2000. \$45 (cloth); \$18.95 (paper).

As you flip through the pages of *The Scopes Trial: A Photographic History*, you may experience thoughts and feelings akin to those of people who discover boxes of old, forgotten photographs. You know something about the contents, but you are curious and perhaps even excited by what you may find. You are not disappointed, as you rediscover familiar faces and ponder faces unknown. You may even be inspired to delve deeper into the stories behind these faces.

Edward Caudill's introduction is an intelligent, in-depth review of this early twentieth-century “trial of the century.” The players are clearly described, and the story unfolds to provide a comprehensive introduction for the novice and a detailed supplemental text for the expert.

The captions by Edward Larson add a third dimension to the black-and-white images that capture the drama and the commercialism that engulfed Dayton, Tennessee, during the “Monkey Trial.” As one would expect, John Scopes, Clarence Darrow, and William Jennings Bryan are well represented in this collection. What is satisfying to the curious mind is the inclusion of photographs of George Rappleyea, Fred Robison, and others who planned to put Dayton on the map by challenging Tennessee's antievolution statute. Included among the faces of statesmen and scientists are the numerous nameless folks who offer a glimpse into the social aspect of the trial. Fittingly, the final photo is of William Jennings Bryan's flag-draped casket as it is being placed on a train to Washington, D.C. Larson's words provide more than a description of persons, places, and things. They become a narrative that takes readers to Dayton during those hot July days in 1925, making us aware of the human elements in this “battle” between religion and science.

The afterword by Jesse Fox Mayshark could stand alone as a brief account of the creation/evolution debate in recent history. Mayshark skillfully describes the roles played by religion, politics, and regionalism in this seemingly endless controversy. He offers an insightful report on the stigma attached to the educational system in Tennessee and the influence of antievolution laws on faculty, administrators, and textbook publishers.

The Scopes Trial: A Photographic History is a rich source of material about an event that took place over seventy-five years ago. It is appropriate for high school students, undergraduates, or graduate students. The general reader would also find this book to be of great interest.

As I read the book and studied the photos, I was reminded of words spoken by Bill Moyers as he introduced a television program dealing with twentieth-century history. His general point was that videos allow the viewer to live simultaneously in the present and in the past. If this is true, then surely the same applies to photographs, for they allow the viewer to be present at the moment when the photograph was taken.

PAUL J. CECHE

Christopher D. Green; Marlene Shore; Thomas Teo (Editors). *The Transformation of Psychology: Influences of Nineteenth-Century Philosophy, Technology, and Natural Science*. xviii + 245 pp., illus., figs., tables, bibls., index. Washington, D.C.: American Psychological Association, 2001. \$39.95.

This volume is a collection of essays presented during a series of seminars at York University in Toronto in 1998–1999. The seminars' goal was to assess the evolution of the field of psychology from its philosophical character at the beginning of the nineteenth century to its scientific identity at the end of the century. Although they deal with terrain familiar to historians of the human sciences, the essays offer innovative insights about the range of influences that surrounded the launching of professional psychology.

The book confronts the conventional wisdom about the succession of great founding individuals and adds the impacts of “various cross-currents of thought in philosophy, science, technology, and institution-building [and popular culture] that led a wide array of people to render psychology scientific” (p. xvi). By contextualizing the emergence of scientific psychology, this volume both provides a good example of externalist historical methodology and shows the

importance of scientific thinking in the history of the human sciences.

The essays are of three types. Many describe the meaning of psychological and scientific practice in their own time. Raymond Fancher sets a broad cultural and intellectual context for the emergence of psychology within the circles of naturalistic thinking fostered by the advent of Darwinism; with his case study, Fancher tracks the evolution of Francis Galton through increasingly pantheistic ideas to the development of eugenics as a kind of alternative religion. Marlene Shore explores the social applications of memory research by evaluating the appeal of psychology at the World Columbian Exhibition at a time of increased concern for “the past’s erasure” (p. 64), as rapid social change posed a threat to cultural memory. John Benjafield examines the intersection of mathematics and psychology in the work of J. F. Herbart and Gustav Fechner to evaluate the aesthetic appeal of geometric proportions such as the golden section. Christopher Green shows that Charles Babbage’s and Ada Byron Lovelace’s well-known work in anticipation of computing machines was conducted without an intent to support scientific reductionism in the understanding of mental functioning. Katherine Anderson explores the boundaries between the natural precision of animal instincts and the hopes for mechanical precision with the increasing use of (and faith in) instrumentation.

A few of the essays report on how features of modern psychology came to be. By focusing on the theory and uses of memory by two early psychologists, Kurt Danziger shows that Wilhelm Wundt’s experimental investigations had a social relevance far less broad than the work of Hermann Ebbinghaus, which was more useful for practical work with memory, especially in education. Andrew Winston examines Ernst Mach’s shift from causal to functional explanations, which would at once simplify descriptions of mental phenomena, make them more amenable to scientific treatment, and open them to use by political agents for social change. Frederic Weizmann surveys the evolution of arguments for the influence of internal and environmental factors in prenatal development and points out that even as genetic arguments held sway, they adapted the environmental view of “critical periods” of fetal development.

Other essays consider paths not taken, either to show their relation to current practice or to offer critiques of the psychological mainstream. Michael Sokal is willing to bracket questions about the truth of phrenologists’ ideas in order to assess their social function as popular coun-

selors. In Charles Tolman’s critical essay, the thoroughly empirical practices of modern scientific psychology look philosophically thin in comparison with Kant’s and Hegel’s profound assessments of the moral reasons for human behavior. Thomas Teo points out that Karl Marx and Wilhelm Dilthey offer an alternative, socio-historical view of the mind that contrasts with the predominant individualism of mainstream psychology.

All the essays in this volume provide fascinating and helpful excursions into the historical roots of contemporary psychology.

PAUL JEROME CROCE

E. I. Kolchinski. *V poiskakh sovetskogo “soiaeuza” filosofii i biologii: Diskussii i represii v 20-kh nachale 30-kh gg.* 263 pp., bibls., index. St. Petersburg: D. Bulanin, 1999.

For the past three decades, Western and émigré scholars of Soviet biology have focused on the bizarre career of Trofim Lysenko, Stalin’s charismatic leader of the campaign against genetics and evolutionary biology that reached its zenith in an August 1948 session of the Lenin All-Union Academy of Agricultural Sciences, which banned those sciences in the USSR. E. I. Kolchinski began to study philosophical questions in the history of biology when *glasnost* first encouraged an internal rediscovery of the past in the USSR. In this book he examines the social and political conditions that made Lysenkoism possible. Dubbing it “the ugliest result of the Stalinist regime” (p. 262), he argues that it emerged in the wake of attempts in the 1920s and 1930s to develop some sort of “proletarian” or “dialectical” biology.

The Russian intelligentsia saw the Bolshevik seizure of power as a catastrophe. Nevertheless, the Bolsheviks supported science at an unprecedented level, viewing it as an instrument for transforming nature and society. During the period of the New Economic Policy (NEP) in the 1920s, they undertook to reshape its ideological base. To that end, they created new establishments, including Communist universities and an Institute of Red Professors. Those newly trained biologists who adopted a dialectical materialist perspective aggressively accused their opponents of vitalism, mysticism, idealism, teleology, and a devotion to “bourgeois science.” Still, the best professional biologists held their own against the vulgarizing Marxists.

Between 1928 and 1932, during the “cultural revolution” when Stalin was promoting his second revolution, the Bolsheviks upped the ante

and promoted a faith in quick-acting methods of agricultural development. Kolchinski shows how the establishment threw its support behind the philosopher Abram M. Deborin, who championed constructing natural sciences on the basis of dialectical materialism. For this purpose, they employed the state security apparatus and formed special brigades to scrutinize specialists. In the end, however, neither the NEP nor the cultural revolution achieved the Communist Party goal of creating a proletarian biology.

So there began a liquidation of earlier organizations and journals designed to introduce Marxism into biology. In 1931 the Deborinists were accused of capitulating to bourgeois science and their work was declared anti-Marxist. Although some promoters of dialectical materialism repented of “political and philosophical mistakes,” that did not save them from later being purged.

Kolchinski devotes considerable attention to Isaak I. Prezent, who emerged triumphant in this ideological contest. A member of the philosophical section of the Leningrad Research Institute for Marxist-Leninist studies, Prezent established contact with Lysenko in 1932. Thereafter he provided the philosophical and ideological glue that held together Lysenko’s disastrous notions concerning breeding and inheritance.

Some people attempted to resist the encroachment of ideology into science. Among them was the eminent mineralogist Vladimir I. Vernadsky (Kolchinski labels him a physiologist), a political liberal and an implacable critic of Bolshevism, who between 1922 and 1926 worked in both Paris and Prague. Then, unaccountably, he went home. In 1992, one of Kolchinski’s students consulted Vernadsky’s private papers in the Bakhmeteff Archive of Columbia University. In a supplement to his book Kolchinski asks, “Why did V. I. Vernadsky return to the USSR?” and answers that Vernadsky was lured back by the Bolsheviks’ generous support for science.

Notwithstanding its clumsy twelve-page English-language summary, Kolchinski’s book offers an instructive analysis of the vagaries of totalitarian ideology and a valuable addition to the study of the history and philosophy of Soviet science.

ELIZABETH V. HAIGH

Richard Weikart. *Socialist Darwinism: Evolution in German Socialist Thought from Marx to Bernstein.* Foreword by Alfred Kelly. x + 257 pp., bibl., index. San Francisco/London: International Scholars Publications, 1999.

Two decades ago, Alfred Kelly included a still-valuable chapter entitled “Darwin, Marx, and the German Workers” in his study on the popularization of Darwinism in nineteenth-century Germany (*The Descent of Darwin* [North Carolina, 1981]). In his foreword to Richard Weikart’s book, Kelly rightly praises the author for providing the first detailed analysis of how leading German socialist thinkers came to terms with Darwinism. Weikart devotes individual chapters to five influential socialists—Karl Marx, Friedrich Engels, August Bebel, Karl Kautsky, and Eduard Bernstein—and adds observations on two non-Marxian authors who maintained sympathies for the socialist movement: the materialist Ludwig Büchner and the neo-Kantian philosopher Friedrich Albert Lange.

In sober and lucid prose Weikart demonstrates that these thinkers did not simply project Darwinian ideas onto societal processes and thus “biologize” social theory; rather, they used some elements of Darwinian biology (and left out others) in their attempts to develop a scientifically grounded socialist theory (with Lange and Büchner having less ambitious aims). Marx and Engels are cases in point: Marx himself remained fundamentally ambivalent about Darwin and did not apply biological mechanisms to social life and morality, drawing a sharp line between animals and human beings. At the same time, he instrumentalized Darwinism as a weapon against idealism and clericalism and expanded his understanding of the universal meaning of laws into the social realm under the influence of Darwin. Weikart shows that for a while Marx flirted with the ideas of the French author Pierre Trémaux, who emphasized the role of environmental factors, in particular the soil, on evolution. Engels, much more at home in the natural sciences, went beyond Marx in stressing the unity between nature and human society, both driven by dialectical laws that Engels formulated by synthesizing Hegel and Darwin. In his biological views, however, Engels remained closer to Lamarck than to Darwin.

This last observation marks the actual limits of the reception of Darwinism among German socialists as well as of the validity of Weikart’s argument about a “socialist Darwinism.” In fact, all leading socialist thinkers rejected Thomas Malthus’s population theory, which was crucial for Darwin, as well as the accompanying principle of the struggle for existence, a key element of Darwin’s evolutionary thinking; they refused to accept any notion of a necessary social inequality or competition among humans. Conversely, as Weikart shows in an additional chap-

ter, most German biologists and Darwinists remained in opposition to the socialist camp. Even more, I wonder whether “socialist Darwinism” really is an adequate label for what was essentially a very pragmatic and eclectic use of Darwin and a propensity toward Lamarckism, with its stress on the environmental influences on evolution and the inheritance of acquired characteristics. The leading socialists were Lamarckians rather than Darwinians—certainly anti-Malthusians; they were more dialectical than the ardent materialists such as Büchner and Karl Vogt; and sometimes they even favored eugenic ideas, without accepting the notion that the environment had no effect on heredity, as Weikart explains in a strong chapter on August Bebel. Against this background, Weikart’s point that Darwinism, after all, did lend support to specific social views and should not be deconstructed into endless polyvalences, as recent historiography has tended to do, remains worthy of further discussion.

Weikart’s study is of particular value for professional historians. Students without much knowledge in the relevant fields might wish for more social and political contextualization—not only to understand the actual relevance of the extremely ardent arguments about Darwinism among contemporaries but also to examine how ideas were actually shaped by social realities beyond the intellectual world of the individuals who formulated them.

ANDREAS W. DAUM

Helena Gourko; Donald I. Williamson; Alfred I. Tauber (Editors). *The Evolutionary Biology Papers of Elie Metchnikoff*. (Boston Studies in the Philosophy of Science.) x + 221 pp., figs., indexes. Dordrecht/Boston/London: Kluwer Academic Publishers, 2000. \$143, £89, Nlg 270.

In 1864 Elie Metchnikoff graduated from Kharkov University with several published papers in zoology to his credit. A government stipend sent him to Klagenfurth and Giessen for advanced studies in comparative zoology. He also acquired valuable research experience at the famous marine research stations in Messina and Naples.

When *The Origin of Species*, Darwin’s biological classic, was translated into Russian in 1865, the ideas of biological evolution became the main research attraction to the rapidly growing ranks of Russian naturalists. From the late 1860s to the early 1880s, Metchnikoff was a professor at Odessa University, where he specialized in embryology and applied its methods to

the study of selected invertebrates, some from the Mediterranean habitat. One of his basic aims at the time was to unveil the hidden similarities between the embryonic layers of vertebrate and invertebrate and thereby give firmer footing to Darwin’s assumption of the unity of the animal world.

Metchnikoff also took time to publish his mainly enthusiastic comments on Darwin’s evolutionary ideas. But his lavish praise of Darwin did not prevent him from pointing out what he considered to be the major shortcomings in his reasoning. One such flaw was the “Malthusian bias” in Darwin’s conceptualization of the struggle for existence. At an early point in his professional career, Metchnikoff believed that science commanded so few facts on the struggle for existence that it was forced at every step to rely on indirect “proofs” and logical deductions. In his criticism of Darwin’s conception of the struggle for existence, Metchnikoff relied heavily on the use of anthropological arguments and on the floating of generalizations on social and cultural evolution. Nevertheless, in Metchnikoff’s view Darwin’s theory of biological evolution was essentially correct; its only weakness, as he saw it, was that it did not go far enough, a task left for future generations. At the end of his very active scientific engagement, Metchnikoff came close to accepting natural selection as the main engine of the evolutionary process.

The book under review consists of a summary of chapters selected from *Metchnikoff and the Origin of Immunity* (published in 1991 by Alfred Tauber and Leon Chernyak) and an English translation of ten articles written by Metchnikoff. The articles form two distinct groups: scientific papers published by Metchnikoff in such professional journals as the *Zeitschrift für Wissenschaftliche Zoologie*; and articles meant to reach a wider audience, published in such journals as *Vestnik Evropy* [*The Herald of Europe*]. The popular articles were not immune to sociological speculations.

Endowed with both a keen mind and an unusually fertile imagination, Metchnikoff placed Darwin’s evolutionary thought in a broader perspective than did most of his leading professional contemporaries. Metchnikoff’s commitment to the philosophy of rationalism made him a staunch and consistent critic of vitalism, an anti-Darwinian orientation with metaphysical roots. Metchnikoff saw science as an embodiment of rationalism and hence as the primary wheel driving modern cultural progress.

Metchnikoff’s evolutionary ideas, built mainly on embryological foundations, passed

through a process of continuous and severe criticism from scientists such as Ernst Haeckel, who were inclined to make grand evolutionary generalizations without adequate empirical support. Continuous readjustment to the flow of new biological ideas led Metchnikoff in 1882 to a drastically new orientation, which he labeled the “phagocytosis theory.” The newly discovered phagocytes, identified as wandering microbes, performed a dual function: they played a key role in intercellular digestion and they destroyed pernicious intruders. As Metchnikoff stated much later, he was suddenly transformed from a zoologist to a pathologist. In 1908, as a “pathologist,” he received a Nobel Prize, an honor he shared with the eminent German bacteriologist Paul Erlich.

ALEXANDER VUCINICH

[EDITOR’S NOTE.—Alexander Vucinich passed away in May 2002 after composing this review. Reginald Zelnik kindly completed and submitted it.]

Nathaniel C. Comfort. *The Tangled Field: Barbara McClintock’s Search for the Patterns of Genetic Control*. x + 337 pp., illus., bibl., index. Cambridge, Mass.: Harvard University Press, 2001. \$37.50, £25.95 (cloth).

Nathaniel Comfort reexamines the story of Nobel laureate Barbara McClintock’s life and work, which Evelyn Fox Keller first published in *A Feeling for the Organism* (Freeman, 1983). Keller’s memoir argues that McClintock was a brilliant female scientist who, unlike her male contemporaries, could not find permanent employment in the early part of her career. Her revolutionary work on transposition, conducted in relative isolation at the Carnegie Institution’s Department of Genetics at Cold Spring Harbor, was initially rejected because it overturned the established paradigm that genes were fixed on chromosomes. When male scientists demonstrated transposition in bacteria, McClintock’s ideas were acknowledged and ultimately recognized with the 1983 Nobel Prize in Physiology or Medicine. Comfort dismantles this story by examining the investigations that led McClintock to her discovery of “controlling elements” during the 1940s; he focuses on the intricacy of McClintock’s character and the complexity of her scientific accomplishments.

Reinterpreting interviews McClintock offered to Keller and to others, Comfort takes apart the popular story, which he calls McClintock’s “private myth.” By comparing those interviews with McClintock’s unpublished research notes and

correspondence and with his own interviews with McClintock’s colleagues, he elucidates the details of her life. He views McClintock’s earlier and later life and work with a “low-power lens,” then offers a closer look at the development of her theory of controlling elements with a “high-power lens.” In a compressed (low-power) view of McClintock’s first forty years, Comfort exposes McClintock’s need for freedom as her “private myth,” which he describes as “a version of her past with which she could be comfortable” (pp. 18–19). With attention to both the social and the scientific milieu, he offers a summary of McClintock’s professional training and early accomplishments, which he recognizes as important for the foundation of her later work. He compares her early studies on corn to those on fruit flies, relying on McClintock’s own stories to reconstruct the events that led her to identify and correlate chromosomes with linkage groups, to apply pachytene-stage chromosomes to explain corn sterility, to “discover” ring chromosomes, and to demonstrate the development of nucleoli in corn. In attempting to clarify a conflict between stories and documents, Comfort speculates that McClintock left her position at the University of Missouri for a job at Cold Spring Harbor for personal reasons.

While describing the discovery of transposition and controlling elements, Comfort provides an extensive (high-power) examination of McClintock’s science. Through his themes of pattern, control, and complexity, he emphasizes that her primary research was on the genetic control of development. In these chapters he follows the steps that led from ring chromosomes to the breakage-fusion-bridge cycle and ultimately to transposition, as she searched for the source of developmental control. He reconstructs McClintock’s hypothesis and experiments by correlating that work with materials from her published papers as well as unpublished research notes, notebooks, card files, and correspondence from the American Philosophical Society and other archives. These documents reveal unpublished data, which she relied on for her conclusions and shared with select members of the genetics community. Scientists accepted the mechanism of transposition, Comfort explains, but questioned the proposal that movable elements controlled development. Stepping back from his close examination of McClintock’s papers, he reasons that the reception of her work on transposition, which ultimately resulted in the honor of an unshared Nobel Prize, was not her long-awaited legitimization. That award, he explains, was not for controlling elements, the con-

cept that she considered to be her primary contribution (see Ch. 9, "Renaissance").

Comfort's interpretations based on documents found in archives are probably more reliable than his interpretations founded on interviews. For example, he integrates McClintock's early career into the history of genetics by repeating her memories and citing her published papers, yet he perpetuates the misconception that, as a young graduate student, she used two innovative techniques to solve the problem of morphologically identifying the chromosome number in corn. Her solution, he reiterates, was to adapt a new squashing technique to corn and view the chromosomes at mid-prophase of meiosis, a phase called the pachytene stage. An accurate reading of McClintock's papers (and those of her contemporaries), however, clearly shows that she used the late mitotic prophase in the microspore and observed unpaired (haploid) chromosomes to identify the corn chromosomes. Concurrently, she initially applied this technique to associate a number of genetic linkage groups with their chromosomes; only later did she employ pachytene-stage chromosomes to confirm those findings. Additionally, Comfort's rendition of how McClintock "discovered" ring chromosomes is unintentionally humorous, the result of his reliance on interviews. Comfort clarifies that McClintock's discovery of these chromosomes was inspired by a paper she received in 1931 from "Susumo Ohno," whom she then visited at his Berkeley laboratory. Readers may be amused to learn that Ohno was two at the time. The similarity of names led transcribers to confuse Ohno's name with that of H. Olmo, a graduate student in E. B. Babcock's group at Berkeley. A more careful reading of McClintock's published accounts clearly demonstrates that her thoughts on ring chromosomes were inspired by descriptions and illustrations of ring chromosomes previously published, in 1930, by M. Navaschin, a researcher associated with Babcock.

Comfort's book provides a different and perhaps discomfiting interpretation of Barbara McClintock's work on transposition and controlling elements in maize. His lens metaphor reflects a sweeping examination of available documents for the 1940s but suggests only a limited inspection of accessible material for her early and late years. His curbed approach to the latter seems to have resulted from placing his doctoral dissertation research (which examined the discovery and reception of McClintock's controlling elements) into a larger framework.

L. B. KASS

Matthew Pratt Guterl. *The Color of Race in America, 1900–1940*. 256 pp., illus., notes, index. Cambridge, Mass.: Harvard University Press, 2001. \$39.95 (cloth).

In the past decade a number of historical studies have focused on the creation of the "white race" in the United States, showing how a number of "races" (for example, Slavs, Irish, Celts, and others) gradually merged into an undifferentiated "white" race. The disappearance of these different races served to solidify the black/white binary of American race relations in the twentieth century.

The Color of Race in America follows in this tradition of whiteness studies and is a careful and thoughtful examination of four major figures in the intellectual construction of whiteness in the first half of the twentieth century: the Nordicist Madison Grant, the Irish-American nationalist Daniel Cohalan, the African-American scholar W. E. B. Du Bois, and the biracial novelist Jean Toomer. What unites these four thinkers, argues Matthew Pratt Guterl, is their focus, to one degree or another, on racial nationalism. Readers of *Isis* will find Guterl's examination of the work of Grant and Du Bois most central to the history of science. While Guterl notes the influence of scientific debates about the status of the Celt on the political activist Cohalan and the impact of Darwinism on the novelist Toomer, science was not the central concern of either of these figures. By contrast, Grant and Du Bois were central figures in the race science of the early twentieth-century United States.

The nation's premier Nordicist, Madison Grant, was particularly concerned with the immigration to the United States of Southern and Eastern Europeans, especially Jews, in the 1910s and 1920s. After the passage of the 1924 Immigration Restriction Act, for which he had campaigned long and hard, Grant turned his attention to the danger of the "Negro." Guterl carefully traces how Grant's concerns arose not just from his victory in restricting immigration but also in response to the effects of the Great Migration of African Americans from the American South to northern cities. Guterl slights, perhaps, just how important Grant was as an organizer of, and contributor to, American scientific racism. For example, he does not discuss Grant's role in the formation of the Galton Society—his answer to the increasing racial egalitarianism in American anthropology represented by the rise of Franz Boas and his students. Even so, Guterl's work does a great deal to restore Grant as a major figure in U.S. intellectual history, a post long de-

nied him because of his now-discredited racist views.

If Grant has been ignored as a historical figure, the same cannot be said about W. E. B. Du Bois. As one of the nation's most brilliant and iconoclastic intellectuals, Du Bois has been the subject of innumerable historical studies. By contrasting Du Bois with figures such as Grant and Cohalan, however, Guterl gives us new insight into his thought, showing the relationship between Du Bois's Pan Africanism and the Nordic nationalism of Grant and the Irish nationalism of Cohalan. This is especially clear in Guterl's discussion of the strange symbiotic public relationship between Du Bois and Grant's chief disciple, Lothrop Stoddard. Du Bois and Stoddard appeared together in a series of public debates in the 1920s, each respecting the other's intellectual acumen and defenses of their respective races. Guterl does an excellent job of highlighting the similarities between black and white nationalism here and in his discussion of Marcus Garvey's relationship with various white supremacist figures.

The boundaries between politics and race science are nearly nonexistent. Any historian writing on the scientific study of race in the first half of the twentieth century in the United States should be familiar with Guterl's book, as it clearly lays out the political issues that engaged the times. Just how those issues shaped the science of the time is a story still waiting to be fleshed out.

JOHN P. JACKSON, JR.

Richard M. Gale. *The Divided Self of William James*. x + 364 pp., index. Cambridge/New York: Cambridge University Press, 1999. \$70.

The Divided Self of William James is an extension and elaboration of Richard M. Gale's earlier articles on James's views on topics such as the self, time, freedom, ethics, mysticism, and the nature of belief. Gale provides an appreciative and critical analysis of tensions encountered in the philosophical and psychological thought of William James. Specifically, topics such as free will versus determinism, unification versus pluralism, and realism versus ontological relativism are explored, but the major focus is on the tension between what Gale calls "the Promethian pragmatist" and "the anti-Promethian mystic." The Promethian pragmatist seeks totality in a comprehensive and expansive philosophy "that would enable us, as the beer commercials enjoin, to have it all, to grab for all the gusto we can" (p. 4). Gale argues that the quest for such a philosophy is played out both in James's personal

life and in his technical intellectual work. The anti-Promethian mystic—the other side of James's personality—sought more poetic, mystical, intimate, intuitive, and spiritual relations with the world.

Scientists and historians of science understand the tensions between the Promethian pragmatist and the anti-Promethian mystic. Gale notes that what is most intimate and real to the mystic is but illusion, secondary, ephemeral, and epiphenomenal in the scientific scheme of things, a scheme characterized by I–It relations. The scientific world of abstract impersonal concepts of form, mass, number, movement is alien to the more romantic, experiential, and commonsense view of life, a view characterized by I–Thou relations. Gale's empathic treatment of this tension is itself recommended reading.

The overall scheme of Gale's work is to set forth the divisions and tensions he encounters in James's philosophy and then to seek a more unified one-world interpretation of James that brings all of the divided selves into closer harmony. Gale admits that his work slips into an interpretation of "what-James-ought-to-have-said-but-really-didn't" (p. 305) and that his attempts to unify James's many worlds will likely prove overly promissory if not impossible. In a better-ordered world, James and Gale would have been contemporaries so that James could have helped judge the adequacy of Gale's rescue efforts. James was in fact deeply interested in the quest for unities, but his emphasis was largely on practical unities such as unity of language or unity of purpose. It was a mature and more integrated William James who was deeply suspicious, intellectually and temperamentally, of any kind of through-and-through universe or any kind of theoretical unity that ties everything together in a timeless glut of oneness. In his mature philosophical works, James repeatedly saved his most vitriolic rhetoric for monistic philosophies that, in his opinion, did violence to the pluralistic, complex, tangled, and muddy world of experience. It is difficult to imagine that James, even as a so-called anti-Promethian mystic, would have appreciated Gale's one-world interpretation.

It is Gale's hope that *The Divided Self of William James* will appeal to professional philosophers and to a wider audience. My view is that Gale's book makes an important contribution and that professionals who are well acquainted with James will find much in it to appreciate and criticize. It is not, however, a book for those not already familiar with the psychology and philosophy of William James. Highly technical schol-

arly chapters that Gale admits can be omitted without doing damage to an appreciation of the overall mission of the book are not the only problem. A major problem surfaces with Gale's sense of humor, manifested in repeated use of metaphors that distort and exaggerate James's views. James, in his Promethian mode, is described as "an experience junkie intent on having as many tingles and thrills as possible" (p. 32). He is described as a hipster, a philosophical politician, and a metaphysical Pooh-bah (derived by Gale from the character in *Mikado* who holds all offices of the state and speaks first from one perspective, then from another). Some of the colorful metaphors border on caricature. Nevertheless, this work takes its place as a significant and fresh contribution to the technical literature on William James.

WAYNE VINEY

Robert B. Baker; Arthur L. Caplan; Linda L. Emanuel; Stephen R. Latham (Editors). *The American Medical Ethics Revolution: How the AMA's Code of Ethics Has Transformed Physicians' Relationships to Patients, Professionals, and Society*. (Based on papers presented at the AMA sesquicentennial conference "Ethics and American Medicine: History, Change, and Challenge," Philadelphia, 14–15 March 1997.) xl + 396 pp., table, apps., bibls., index. Baltimore/London: Johns Hopkins University Press, 1999. \$59.95.

In the spring of 1847, 268 physicians from around the United States gathered in Philadelphia for the first meeting of a new national medical organization, which they agreed to name the American Medical Association. These delegates also unanimously approved a Code of Ethics that had been prepared by several prominent members of the group. A hundred and fifty years later, a collection of philosophers, physicians, lawyers, historians, and others with an interest in bioethics gathered again in Philadelphia to commemorate the first AMA meeting and the inaugural Code of Ethics. The essays published in *The American Medical Ethics Revolution* were initially prepared for presentation at this 1997 conference.

The conference title, "Ethics and American Medicine: History, Change, and Challenge," is a more appropriate collective label for the twenty essays published in this volume than the chosen book title, which suggests a tight focus on the 1847 code. The volume title is, in fact, a fitting description for only about seventy pages of the book: an essay by Robert B. Baker entitled "The

American Medical Ethics Revolution" (sound familiar?) and the introduction, which also seems to have been penned largely by Baker (who apparently had more than an alphabetic claim on his listing as lead editor). Baker offers a detailed and insightful analysis of the maneuverings—both political and philosophical—behind the preparation of the 1847 Code of Ethics. He has based this aspect of his writing on careful historical research, primarily in the papers of early AMA member Isaac Hays, which are held in the archives of the American Philosophical Society and the College of Physicians of Philadelphia. Perhaps most significantly, Baker demonstrates as false the long-standing interpretation—first put forward by Chauncey Leake in the 1920s—that the 1847 AMA code was more concerned with etiquette (relationships with other healers) than with ethics (relationships with patients); Baker shows that the code dealt with both aspects of medical practice.

The subtitle of the volume implicitly promises an extensive examination of the dramatic practical impact of the 1847 AMA code. Readers turning to this book for such information will almost surely be disappointed. The basic problem, I believe, is that this 5,283-word statement simply never "Transformed Physicians' Relationships to Patients, Professionals, and Society." Such might have been the aspiration of those who prepared the code, but the AMA was a weak organization for roughly a half-century after its founding. And as John Harley Warner clarifies in a very good essay in the volume (based largely on his 1991 *Isis* article, for which he won the Derek Price Award), the 1847 code became increasingly unpopular among more scientifically inclined American physicians in the late nineteenth century. Then, in 1903, the AMA replaced the 1847 Code of Ethics with the substantially different Principles of Medical Ethics. Rosemary A. Stevens, in another excellent essay in the volume, points out (as others have done) that the AMA first "became a national force" during the initial two decades of the twentieth century (p. 88). Thus the widespread impact of the AMA (and any statement it might have produced) would come after the organization had set aside the 1847 code.

In the volume's introduction the editors (presumably with Baker in the lead) assert that during the latter half of the nineteenth century the 1847 Code of Ethics "was the most commonly printed medical document in the English language" (p. xxvii). There is no evidence offered for this bold claim in the introductory essay or elsewhere in the volume. Even if it is true (I have

my doubts), this does not automatically mean that these words held transformative power in nineteenth-century America—and it certainly does not make the 1847 Code of Ethics the pivotal document in the history of medical ethics in America. Indeed, one can write very interesting pieces on the topic with nary a mention of the 1847 AMA code—which is precisely what several contributors to this volume have done.

JON M. HARKNESS

Geoffrey Cocks. *Psychotherapy in the Third Reich: The Göring Institute*. Second edition, revised and expanded. xx + 462 pp., illus., apps., bibl., index. New Brunswick, N.J.: Transaction Publishers, 1997. \$29.95 (paper).

On its first publication in 1986, Geoffrey Cocks's *Psychotherapy in the Third Reich* presented a fascinating thesis: psychotherapy not only survived in the Nazi era; it achieved unprecedented levels of recognition and professionalization. Cocks's work demonstrated that the history of scientific fields apparently peripheral to the Nazi project—such as psychotherapy or the Nazi war on cancer, subsequently described by Robert Proctor—could be used to redefine our understanding of daily life and research in the Nazi state. Fields such as psychotherapy existed at the juncture of institutional and scientific history, personal and public life, and revealed how German scientists often promoted the Nazi state through “legitimate” research agendas that were less centrally controlled, and more self-policing, than had previously been understood. Reorganized to form a chronological narrative, the second edition of Cocks's book tells this story in a manner accessible to nonspecialists and anchored in the social history of the period. New chapters trace the postwar continuities of the psychotherapeutic establishment, with particular attention to West Germany, and situate the argument within contemporary historiography.

The rise of psychotherapy in Nazi Germany occurred as part of an institutional and ideological conflict with medical psychiatry. By definition, the latter adopted the position that mental illness was largely biological, which in the Nazi context meant hereditary and racial. How, then, was the Nazi state to explain and cure mental illness among its putative racial elite? One answer lay in psychotherapy, which stepped in to provide “humane” treatment for problems ranging from industrial stress to homosexuality among privileged members of the Nazi elite (pp. 252, 287). Even as it elided the brutal con-

sequences and general incoherence of Nazi dogma, psychotherapy rewrote its own history—now placed under the rubric of *Seelenheilkunde*—so as to bypass associations with Freud in favor of what Cocks describes as the earlier “romantic” theories of figures such as Carl-Gustav Carus (p. 47). Although it is not emphasized here, this approach also tacitly linked psychotherapy with contemporary “holistic” theories in Germany that underlay relatively successful research fields ranging from theoretical biology to field theory in physics.

Despite their early complicity in attacks on the “Jewish science” of psychoanalysis and its practitioners, the German psychotherapists nonetheless began to develop internally consistent readings of Alfred Adler, Karen Horney, and Carl Jung that often focused on practical therapies rather than Nazi ideology. Even as the Freudian foundations of modern psychoanalysis were denied, by the late 1930s many central elements of Freud's theories were adopted (p. 276). Although many of the key premises of psychotherapy were retained, their meanings were gradually perverted. Rather than emphasizing inherent flaws and distortions resulting from human psychogenesis, the psychotherapists spoke of lifting the “inhibitions” created by modern society to reveal the “organic” and natural Aryan person (p. 81). Even propaganda could become an unapologetic vehicle for “integrating” the healthy individual into the community (p. 80).

The immense value of Cocks's text lies in the manner in which this complex ideological history is anchored in an innovative history of institutions and the professions. The prosaic ground for the success of psychotherapy under the Nazis lay in its institutional support by the Göring Institute, founded by Matthias Heinrich Göring, the cousin of Reich Marshal Hermann Göring. The catalyst for phases of development in psychotherapy was the process of professionalization of medical doctors and, later, psychotherapists (p. 410). Yet the fate of psychotherapy was not simply dependent on nepotism or bureaucracy; rather, the field's emergent place in the polity, or overlapping bureaucratic chains of control, of the Nazi state was vital (pp. 99, 413). By detailing this complex internal history and its consequences for science, Cocks has provided a crucial window on the daily functioning of Nazi Germany.

GREGORY MOYNAHAN

Richard Schain. *The Legend of Nietzsche's Syphilis*. (Contributions in Medical Studies, 46.)

152 pp., notes, bibl., index. Westport, Conn.: Greenwood Press, 2001. \$62.95 (cloth).

One of the obsessive questions in Nietzsche scholarship is, What was the cause of his breakdown in Turin in 1889 and his eventual death in 1900? His friends said syphilis; his sister said overwork and sleeping drafts. A nasty court case over this issue caused the censorship of his colleague Franz Overbeck's account of his friend's life and death in 1908. In the 1920s the charge that Nietzsche died of syphilis was repeated with the leaking of Nietzsche's hospital records and their magazine publication by E. F. Podach. In "Friedrich Nietzsche's 'Niederschriften aus der spätesten Zeit' (1890–1897) and the Conversation Notebooks, 1889–1895," an essay included in Winfried Kudsus and Bernd Urban's edited volume *Psychoanalytische und psychopathologische Literaturinterpretation* (Wissenschaftliche Buchgesellschaft, 1981), I published the mechanical "writings" and conversations from the period of Nietzsche's hospitalization and madness. Finally, in 1990, Pia Daniela Volz undertook the first full-scale account of Nietzsche's illness with all of the sources at hand.

Who really cares? There is no equivalent discussion about Kant's final illness and death or Heidegger's or Plato's (much about Socrates—but that is another problem). The reason for this contestation is that there is a very early argument that if Nietzsche was syphilitic or insane in 1889 everything he wrote before that would have shown traces. Nietzsche was a cult figure by 1900. You either loved and worshiped him or you feared him. There was little middle ground. The mad philosopher, so the story goes on after 1945, inspired the crazy Nazis to their horrid deeds. Now, we know that there is no straight line between Nietzsche and the Nazis even though, through his devoted sister, they had a role in shaping his public image. The legend of the dangerous philosopher is somehow undercut if it is only the crazy philosopher about whom we have to worry.

Richard Schain, a trained neurologist, attempts to present the case for two models: syphilis and madness. He retrospectively uses the category of schizophrenia (which was coined by Eugen Bleuler only after Nietzsche's death) as his label for "madness." He believes that by postulating this as the "real" cause of Nietzsche's illness, he saves his work from being disposed of as merely that of someone suffering from the final stages of syphilis. Like all those who attempt retrospective diagnosis, Schain tries to squeeze Nietzsche's illness into a model that

provides an explanation for what illness Nietzsche really had. This is a pointless exercise. Unless we have a serum test (Nietzsche died before the Wassermann test was introduced) we cannot eliminate syphilis as the cause of his breakdown—nor can we prove it. Such exercises may be fascinating (Did El Greco suffer from astigmatism?) but are in the end of more interest and use in examining why the question is put than in explaining the person studied.

The real question—and this Schain does address—is what the label of "madman" or "syphilis sufferer" will do to rescue or condemn Nietzsche as a thinker. Schain believes that labeling Nietzsche's illness as endogenous helps us to see it as a reflection of his character or personality structure. But it does free him of the curse of immorality. One of Nietzsche's physicians, who was convinced that he had syphilis, also wanted to do this: he argued that Nietzsche had been infected by contacting the infectious wounded he treated while he was an orderly in the 1860s. He had syphilis but not sex! Schain is in somewhat the same box. Nietzsche was schizophrenic—but we know that they can be creative!

You should still turn to Volz for an account of the illness and death of Nietzsche if you are at all interested in this question. The Schain volume makes a case for a world in which adding one more creative madman to the cause could combat the stigma of mental illness by showing that crazy people too can be creative. This is certainly true, but it has little to do with Friedrich Nietzsche's illness.

SANDER L. GILMAN

Bronwyn Rebekah McFarland-Icke. *Nurses in Nazi Germany: Moral Choice in History*. xvi + 343 pp., bibl., index. Princeton, N.J.: Princeton University Press, 1999. \$35, £21.95.

Bronwyn Rebekah McFarland-Icke's *Nurses in Nazi Germany: Moral Choice in History* is a long overdue and important book that directly addresses the issue of the participation of psychiatric nurses in Hitler's medicalized killing programs that, between 1939 and 1945, resulted in the deaths of more than a hundred thousand patients with mental or physical disabilities. Evidence is meticulously presented to show that nurses actively participated in the efficient and well-organized killing of disabled and mentally ill patients during the Nazi era. Their involvement included compiling patient "death lists," accompanying patients transported to death camps, administering drinks, food, and injections containing lethal doses of medication, and

administering “starvation diets” (in accordance with the state-sanctioned “Starvation Diet Decree”) designed to starve the incurably ill to death so as to “prevent them from being dragged through the war” (p. 211).

How and why did the nurses do this? Why did they not protest? What sense is to be made of a group of professionals who “remained subject to an ethical imperative to heal and promote life in a spirit of charity” (p. ix) and yet participated fully in racially motivated compulsory sterilization and “euthanasia” policies? It is these and related questions that *Nurses in Nazi Germany* seeks to answer. While the book offers plausible explanations for why the nurses involved betrayed the thousands of patients who were dependent on them so terribly, it is careful not to excuse them: the nurses were fundamentally and unequivocally independently responsible and accountable for their own actions.

There is no denying that the social, cultural, and political conditions of the times made psychiatric nursing work difficult. Most psychiatric nurses were undereducated, underpaid, and overworked. They were not highly regarded by the public and were at the mercy of abusive administrators who had the power to dismiss them at will. Lacking union representation and a collective will to improve their own situation, nurses sought refuge in denial. Unable (or unwilling) to advocate for themselves, these nurses were even less able and likely to advocate for the interests of their patients.

It would be easy to characterize this appalling era in nursing history as a profound failure of professional ethics. But as McFarland-Icke correctly points out, this would not be enough. What is also fundamentally at issue here is a failure in the moral character of the nurses involved—their poor attitude, their moral complacency, and, at times, their frank moral turpitude. Refusing to question the status quo, these nurses allowed a politically constructed contempt for psychiatric patients to supplant morality as the ultimate test of conduct and allowed themselves to become hardened to the fate of their charges. McFarland-Icke writes, “They no longer saw individual, suffering patients, but only hopeless cases for whom they could do little and who appeared more and more as ‘useless eaters.’ Pressure from official propaganda and cost-slashing bureaucrats only increased the temptation to accept and deploy radical solutions. Killing these patients simply followed as a matter of course; it was ‘mass murder without a guilty conscience’” (p. 6).

The lessons presented in this work are clear.

All health-care professionals (not just nurses) have a stringent moral responsibility to foster a moral conscientiousness both within themselves and in their coworkers and to challenge institutions and processes that normalize the dehumanization of vulnerable people, routinize inhumanity, foster the displacement of moral and legal responsibility, portray moral argument as a nuisance, neutralize dissent, enable people to take refuge in gaps of accountability, and enable victimizers to maintain a disquieting peace of mind in the face of the devastating harm they have caused. If professionals (nurses included) do not fulfill this responsibility, not only do they risk failing their patients and the broader communities they serve, but they risk failing themselves.

MEGAN-JANE JOHNSTONE

Ruth Leys. *Trauma: A Genealogy*. x + 318 pp., index. Chicago: University of Chicago Press, 2000. \$55 (cloth); \$19 (paper).

Ruth Leys’s remarkably interesting book about the history of the notion of trauma combines the qualities of intelligence, clarity, and scholarship. The author has an excellent knowledge of the international literature. She describes the evolution of theories about trauma before Freud, by and around Freud, and up to the present day, claiming that these theories did not develop continuously but that the problem emerged periodically in connection with significant events: Freud’s discovery of the traumatic origin of hysteria, World War I, World War II, the Holocaust, and the Vietnam War.

Leys organizes the various theories about the nature and the effects of trauma by classifying them according to two main orientations: the mimetic and the antimimetic. Mimetic theories are based on the mechanism of hypnotism: the traumatic reaction is like a hypnotic imitation, an identification with the aggressor’s will, a psychic experience that shatters the cognitive-perceptual capacities of the victim and, in consequence, makes the traumatic event unavailable to recollection. Antimimetic theories center on the idea that the trauma is a completely external event visited on an autonomous but passive subject; thus it can be recalled.

Leys shows that the two orientations, though contradictory, are constantly confused by all the authors, including Freud. As a consequence of this unsolved—and maybe insoluble—dilemma, many questions remain unanswered in their work: Can trauma be recalled, or not? Are there several kinds of memory? Are hypnotic repetitions reliable? Are traumatic and repetitive

dreams veridical or not? Is the goal of treatment to remember or to forget?

After a substantial introduction that describes her own thought process, Leys turns to Freud's changing views about trauma throughout his life. She points to the contradictions in his thinking, showing the impossibility of adhering completely to any one position or of reconciling his various theories.

A long chapter treats the classic case of Miss Bauchamp, described by Morton Prince, which can be considered the model of multiple personality theory. In his theorizing about the case Prince tries to maintain an antimimetic position—but without success, as Leys demonstrates.

The traumas of World War I brought to the fore the problem of cure. The various oscillations and confusions between the mimetic and antimimetic lines brought about similar oscillations and indecision in therapy. Doctors experimented, in turn, with hypnotism, encouragement of emotional abreaction, various techniques to bring about conscious remembering, "moral rehabilitation" through "rational persuasion," and so forth. All of these efforts yielded important observations and improved understanding of traumatic reactions, but none of them could claim significant and systematic therapeutic success.

Next Leys turns to the work of one of the most talented followers of Freud. Sándor Ferenczi was particularly interested in the study of trauma, especially in his last writings and his *Clinical Diary* of 1932. Leys writes: "For Ferenczi, the goal of treatment was to abreact the trauma under conditions of analytic honesty and sympathy so that the patient's neurotic tendency to repetition—a tendency Freud attributed to the radical unleashing of the death drive—could be converted into a conscious recollection of the repressed or dissociated event" (p. 123).

But even Ferenczi had to admit therapeutic failure; the splitting and disintegration of the psychic apparatus has the result that the trauma can never be consciously experienced. Ferenczi supposed that identification is prior to object relation; that is, it happens in a state where all self-defense is excluded. He concluded that the traumatized adult falls back to this state. As in her treatment of other practitioners, Leys highlights the inconsistencies and contradictions in Ferenczi's theorizing and therapeutic attempts not to discredit him, but to show starting points for further research. Ferenczi's ideas prompted the work of Abraham Kardiner, who described the traumatic effect as a regression to the birth situation.

Both Ferenczi and Kardiner adopted the mimetic orientation. Leys uses their work as a basis for studying the mechanisms and forms of suggestion and hypnotism.

Next she considers the researches of William Sargant, an adversary of psychoanalysis who worked on the problem of trauma during and after World War II. He insisted on the somatic character of the traumatic reaction and introduced the term "physioneurosis." His therapeutic experiments aroused many questions: Does abreaction cure because of the emotional discharge it prompts (participatory model) or by causing synthesis and reintegration of the dissociated or repressed memory of the traumatic experience (surgical model)? What can be achieved with various drugs? Are traumatic dreams reproductions of the actual trauma, distorted representations, or sexual-symbolic representations? What is the role of therapy: forgetting or remembering?

The two last chapters of the book deal with the work of two contemporary authors: Bessel van der Kolk and Cathy Caruth. Both take a neurobiological approach. Both consider traumatic symptoms (repetitive traumatic dreams, flashbacks) as "veridical memories or representations of the traumatic event" and as "literal replicas or repetitions of the trauma . . . that as such . . . stand outside representation" (p. 229).

Two interesting contemporary problems are raised in these chapters: the possibility of transmission of the trauma to those who listen to accounts of it and the problem of intergenerational trauma. Caruth considers how past traumas can haunt succeeding generations like a ghost, a subject particularly studied by Nicholas Abraham and Maria Torok. Leys is very critical of both van der Kolk and Caruth. She compares their positions with those of other authors, primarily Freud, and points to their misunderstandings and misuses of various notions. She also reproaches them for the insufficiency and lack of clarity of the observations they report and the weakness of their conclusions.

This long discussion (seventy pages in a book of just over three hundred pages) tends to unbalance this very interesting study. One could regret that Leys pays so much attention to these two authors and so little to the researches of Abraham and Torok, for instance; moreover, the work of Michael Balint is scarcely mentioned. Perhaps she decided to devote an extended discussion to van der Kolk and Caruth because, as she writes, "the theory of the literal nature of traumatic memory continues to gain widespread adherence, even as it remains inadequately for-

mulated and weakly supported by the scientific evidence" (p. 265).

Throughout the book Leys enhances her treatment of the central figures who worked on trauma by offering extremely instructive comparisons with the work of their contemporaries or more recent authors. For instance, she uses the writings of Mikkel Borch-Jacobsen to provide a "contrapuntal" voice.

I have a few quibbles. The abundant use of abbreviations will complicate reading for those not gifted with extremely good memories. A feminist tendency manifests itself in the author's habit of using the feminine "she" whenever the gender of the subject is not clearly defined. On the whole, however, this book offers a remarkable discussion and comparison of the different ways to approach the problem of trauma put forward during the last 150 years.

JUDITH DUPONT

Maureen K. Lux. *"Medicine That Walks": Medicine, Disease, and Canadian Plains Aboriginal People, 1880–1945.* x + 300 pp., illus., bibl., index. Toronto: University of Toronto Press, 2001.

This seminal volume documents the appalling health conditions of aboriginal peoples on the Canadian Plains from the disappearance of the buffalo and settlement on reservations in the late nineteenth century to the end of World War II. Maureen Lux combines ethnohistorical documents with oral tradition passed down to contemporary First Nations elders. Her emphasis on the consequences of imposed neglect and forced assimilation from the Native point of view provides a crucial corrective to the self-serving complacency of most mainstream documents. These substandard (even by the standards of the time) health conditions were rationalized alternatively in terms of racial deficiency and the need for fiscal efficiency in Indian administration.

White observers, including scientists, have focused on population decimation resulting from epidemic disease, a "biological invasion" for which previously isolated North American populations were unprepared. Germs, rather than colonial invaders, replaced conscious genocide in this explanatory framework. This view rests on the long-outdated intellectual scaffolding of social evolution: Indians were unfit to survive and properly disappeared to make room for the expansion of settlement (which further eroded their land base and subsistence opportunities). Inherent racial flaws were rarely questioned as explanations because they fit the political goals of

assimilation, primarily measured by settled agriculture and Christianity.

Aboriginal peoples were articulate but unheeded in protesting that military, cultural, and economic encroachments were at the core of their failure to thrive after reserve settlement. Although the treaties specified provision of "a medicine chest," the Indian Department consistently refused to take systematic responsibility for medical treatment. After the starvation winter of 1884, the Indian Department produced cod liver oil but not rations; Long Lodge asked for "medicine that walks"—that is, fresh meat—as the thing that would restore health to his people. Four decades of population decline followed, due to crowded conditions, poor diet, inadequate clothing, and disease. These deprivations, in dramatic contrast to the self-sufficiency of the Plains buffalo economy, produced a long-term deterioration of health in which the invading culture must be held complicit.

Each of the five substantive chapters begins with a phrase from the logic of the times. "The first time we were starved by the government" opens the discussion of starvation, while "Help me Manitou" foregrounds the healing traditions already in place. "I was in darkness" was the message of the church-run residential schools, but that message was at least partially resisted. Through 1920, aboriginal medical care was "indifferent to human life and suffering" until tuberculosis persuaded the mainstream of "a menace to the [non-Native] community." The details are fascinating, as is the consistency of the colonial racist message and its invisibility to those who failed to provide a standard of living on the Plains sufficient to preserve the health of Native populations.

Change came only when Euro-Canadians, especially immigrants living near reserves, began to fear contamination from disease in Native communities. Draconian policies of isolation, quarantine, and paternalistic micromanagement of aboriginal lives ensued, producing little amelioration of Native health but some decline in the perceived threat to mainstream safety and progress.

Despite the virtually unmitigated grimness of the story she tells, Lux manages to show Native peoples and their spokespersons as active agents. Despite generations of coercive residential school assimilation, cultures have been preserved and communities have adapted to their changed circumstances. Native peoples in Canada still evince appalling health problems relative to the population as a whole. Nonetheless, their populations are growing, health care in-

vestment is increasing, land claims are being settled (providing financial resources for social development), and aboriginal control over aboriginal health care delivery is becoming the norm.

REGNA DARNELL

Martin Lindner. *Die Pathologie der Person: Friedrich Kraus' Neubestimmung des Organismus am Beginn des 20. Jahrhunderts.* 150 pp., bibl., frontis., index. Berlin: Verlag für Geschichte der Naturwissenschaften und der Technik, 1999. €18.

Friedrich Kraus (1858–1936) was a professor of internal medicine. Martin Lindner's booklet studies Kraus's conception of the organism and its context. Kraus attempted to balance reductionism and holism; Lindner describes this balance and traces its roots.

A great virtue of the book is its clarity, in terms of both narrative structure and language. It resembles a well-structured lecture course, with road map, a summary at the end of each lecture/chapter, and propaedeutic repetitions. There are four parts: a biographical sketch, an explication of Kraus's notion of the organism, considerations of contexts, and a discussion. The emphasis is very much on Parts 2 and 3.

Kraus was born in 1858 in rural Bohemia and studied medicine in Prague at the time of Ernst Mach and in Vienna at the time of Ernst Brücke. He was a professor of internal medicine at the Charité Hospital in Berlin from 1902 until his retirement in 1927. Throughout his life Kraus emphasized the role of quantification, such as the measurement of blood pressure, but at the same time he attempted to understand the body as a whole. The title of the 1897 paper that established his reputation, "Fatigue as a Measure of the Human Constitution" (as in "a strong constitution"), exemplifies his approach. In the 1920s he wrote a philosophical work aiming to reconcile the two opposed poles. In his hands, the organism became a systemic assemblage of parts mutually acting upon each other. Food and sense perceptions amounted to stimuli, prompting a reaction in one part of the body that triggered a chain of responses in other parts. In other words, Kraus conceived of the organism (and also of consciousness) as localizable and measurable while simultaneously an interdependent whole. He added a temporal dimension so that the organism did not perpetually return to the same equilibrium but evolved.

Part 3 discusses in turn the various influences on Kraus, from the pure reductionism of Helm-

holtz, who argued that the mechanics of atoms could explain everything, including culture; through various psychological, philosophical, and literary theories of consciousness (Charles Sherrington, Mach, Robert Musil); to evolutionary and philosophical theories emphasizing the temporal dimension. Henri Bergson, for example, rejected the analyzability of life into a series of static pictures, such as in cinematography—a variant of reductionism. Rather, he believed, at every moment the *élan vital* adds something creative. Kraus thought of the organism as continually "becoming" in this Bergsonian sense.

Part 4 will be a disappointment to the Anglo-Saxon historian of science. Kraus is depicted as a static receptacle of various *Zeitgeist*-like currents, not as an individual who actively used the available resources for his immediate aims. There is no attention whatsoever to Kraus's clinical practice, despite the fact that he was put in charge of the effort to "unite research, teaching, and practice" at the Charité (p. 20).

Lindner's genre is that of German philosophical discourse—a discourse that names and categorizes philosophical ideas. It ignores institutional contexts and the possibility that philosophy of science negotiated disciplinary boundaries between religion, psychology, biology, and physics. The gap between German philosophical discourse and Anglo-Saxon history of science can be described in Lindner's own terms: he does not take Bergson seriously. Had he done so, he would have looked for the creative moments in every part of Kraus's discourse, and he may have found opportunism in context.

ARNE HESSENBRUCH

Laura Otis. *Networking: Communicating with Bodies and Machines in the Nineteenth Century.* (Studies in Literature and Science.) 312 pp., figs., index. Ann Arbor: University of Michigan Press, 2002. \$49.50 (cloth).

This book is published in the imaginative University of Michigan Press series "Studies in Literature and Science." It is a commonplace that in the nineteenth century technology affected the way people thought and looked at the world. But how exactly did this happen? This is a large and many-sided question. Laura Otis's book is concerned with one aspect of the question: the concept of network in technology and fiction in the second half of the century. A second theme is the way in which the concept affected the work of some researchers in neurophysiology. However, this is far harder to establish and really requires detailed study beyond the scope of this

book. The problem is that the actual motivating ideas often come from within the subject. For example, Otis mentions Charles Babbage's mechanical notation. But Babbage's interest in notation derived from his earlier work in mathematical notations, and any external influence would be difficult to establish.

Otis is well qualified to tackle the subject. Sufficiently knowledgeable in the nervous system and the history of telegraphy, Otis teaches English literature at Hofstra University. The exaggerated academic division of labor has made studies of this sort difficult, and *Networking* is a welcome attempt to broaden the sometimes rather narrow approach of historians of science and technology.

The joints between the two aspects of Otis's study are here still apparent and the two subjects imperfectly fitted together. The technical study is written as history of science, the study of the novels as English "lit crit," but that is unsurprising given her ambitious task. No one concerned with the history of late nineteenth-century English and American literature should ignore this book. But what is remarkable, neither should students of the history of telegraphy. Certainly no student of George Eliot can afford to overlook Otis's pioneering work. It is to be hoped that others will follow in her footsteps and that further detailed studies of the interrelation between nineteenth-century science and technology and literature will be undertaken.

One might cavil at some details. The description of Ada Lovelace as a mathematician is optimistic. I would have preferred "student of mathematics" (see Anthony Hyman, *Science and Reform: Selected Works of Charles Babbage* [Cambridge, 1989], p. 263). However, in general the book is commendably accurate. Laura Otis writes in a pleasant style and has useful observations to make about the perennially interesting and always important subject of communication. This book will be a valuable addition to the shelves not only of historians of science but of anyone interested in nineteenth-century industrial society.

ANTHONY HYMAN

Myles W. Jackson. *Spectrum of Belief: Joseph von Fraunhofer and the Craft of Precision Optics*. (Transformations: Studies in the History of Science and Technology.) xii + 284 pp., illus., figs., bibl., index. Cambridge, Mass./London: MIT Press, 2000. \$34.95.

Optics, Myles Jackson tells us, was a field long divided between the purest heights of natural

philosophy and the blackened furnaces of artisan glassmakers. In the early nineteenth century, Joseph von Fraunhofer's work on achromatic lenses sliced across this division, both fascinating and troubling a generation of scientific investigators struggling to define the proper place of science. By focusing on various sites of this anxiety, Jackson turns Fraunhofer into a "heuristic tool" to probe the social implications behind the question of what counted as science and what did not.

Jackson makes an important contribution to the ongoing debate in science studies over tacit knowledge by insisting on the distinction between knowledge that is by its nature incommunicable and knowledge that is simply not communicated. Fraunhofer's work may have been difficult to replicate, he suggests, because Fraunhofer made it so. His Optical Institute, after all, was a commercial enterprise centered on the production of the achromatic lenses widely used in telescopes, heliometers, and surveying equipment. Even the dark spectral lines usually associated with Fraunhofer's name were the product of his efforts to produce a better flint glass (achromatic lenses required both crown and flint glass; flint, with a small amount of lead oxide mixed in, was generally the harder to produce). Drawing on long traditions of trade secrecy, Fraunhofer worked hard to build opacity into the lenses that made up his livelihood.

One of the most enjoyable parts of Jackson's text is the meaty historicity he gives this practice of secrecy. We learn, for example, that Fraunhofer located his Optical Institute in the secularized monastery of Benediktbeuern and employed former monks in his glass house. Jackson suggests that the three rules of Saint Benedict (labor, silence, and secrecy) had been built into the monastery's architecture, which maintained a strict delineation between the private, silent space of the monk's cell and areas of public congregation. Fraunhofer continued to enforce strict control over public access, to the point where John Herschel would complain that he had been kept out of the most interesting spaces during his visit.

Yet if this careful secrecy allowed Fraunhofer to dominate the glass market for decades, it also posed the greatest problem to his acceptance within the scientific community. One revealing episode was Fraunhofer's nomination to membership in the Munich Royal Academy of Sciences, which was denied amid accusations that his artisan status and proprietary secrecy prevented him from being a true *Naturforscher*. His observation of dark lines in the spectrum, his

accusers claimed, did not count as a scientific discovery because it involved only a craft-based manipulation of existing materials rather than genuine creation. Behind this, Jackson argues, are shifting notions of scientific authorship that increasingly tended to exclude forms of skilled labor.

Some of the tensest discussion of Fraunhofer's work occurred in Britain. Excise laws left Britain's once-dominant glass industry in a shambles and forced British opticians to tackle head-on the issue of replicating Bavarian glass. Jackson maps the strategies of John Herschel, David Brewster, and Michael Faraday onto a set of broader concerns over the status of artisanal knowledge. Where Brewster emphasized the importance of craft skill in producing glass (and worked to establish schools of mechanical training), Herschel claimed that the answer lay in the correct application of the scientific method (and criticized craft secrecy as antithetical to scientific openness). Faraday, after continually failing to reproduce Fraunhofer's results, eventually gave up and blamed "the many years of practical life" needed to make quality glass. This anecdote shows as well as any how tacit knowledge gets written into histories of craft practices.

THERESA LEVITT

Harry G. Lang. *A Phone of Our Own: The Deaf Insurrection against Ma Bell.* xviii + 242 pp., frontis., illus., app., bibl., index. Washington, D.C.: Gallaudet University Press, 2000. \$29.95.

Nearly every advance in communications technology has been for the hearing only. This book recovers the story of ordinary deaf inventors who bent the telephone network to the needs of the deaf through a new service, TTY (teletypewriters for the deaf).

The heroes of this book—and it is largely written in the heroic style—are the hearing impaired themselves. Robert Weitbrecht, a physicist, teamed with James Marsters, an orthodontist, and Andrew Saks, an electrical engineer and grandson of the founder of Saks Fifth Avenue. Beginning around 1963, they sought ways of turning the sound impulses of the telephone into written text that the nonhearing could read.

Teletype technology had long existed and was a lucrative, high-end business service marketed by AT&T and Western Union to big corporations, the military, and the government as TWX (teletypewriter exchange service). The machines of the pre-electronic era were large, loud, and subject to mechanical breakdown. But Weitbrecht acquired one and devised his own modem

to connect it to the public telephone network. He helped other deaf people set up their own machines, using cast-off commercial models slated for destruction. As with any interactive network, growth was slow. Potential users might value teletype, but until there was a sufficient base of users the machines had a limited reach.

The book concentrates on Weitbrecht, his partners, and their struggles to develop the necessary hardware, Weitbrecht's modem in particular. But the back story tells us also of ordinary deaf people who, through various means, found out about the technology on their own. User groups, often assisted by Weitbrecht, recovered the machines, learned how to repair them, and began forming the first TTY networks. Harry Lang largely ignores another aspect in the diffusion of this technology—the political backing that made telephone companies provide operators who could "translate" between deaf TTY users and hearing telephone users. That happened late in the game and largely through federal intervention, particularly the efforts of Senator Tom Harkin, whose brother Frank was deaf. But Lang's book is more about the early days, and it also connects the development of TTY to the stronger activism by the deaf—and, more broadly, all the disabled—that emerged in the 1970s and 1980s.

This focus tends to overplay the heroic nature of the early struggle and requires, as all heroic tales do, an enemy. The enemy is identified in the book's subtitle: Ma Bell, or the old, predilecture, Bell System. Getting teletype machines into the hands of the deaf ran against company interests. AT&T had long argued that it had the sole right to attach equipment to its network. It controlled attaching technology by leasing rather than selling it, including teletype machines. Old teletypes were destroyed to prevent their entering a gray market of illegal attachments. In the 1968 *Carterfone* decision, the courts effectively ended Bell's control of attachments. But AT&T largely left Weitbrecht and his partners alone, even though their acoustical modem would have been a clear violation of the company's pre-*Carterfone* policies.

Lang is on somewhat firmer ground in charging the telephone company with failure to perform more research on technology for the nonhearing. In a book about deaf self-empowerment, though, this charge is a bit strained. More important, it reveals the book's major limitation. As a heroic story, it will admit no irony or unintended outcomes as it pushes forward. Lang assumes that TTY was the one true path to deaf access and faults actors of the time for not seeing

the future. The possibility that other technologies might have seemed equally or even more promising has no place in this teleology.

In all, *A Phone of Our Own* is a useful contribution to the history of communications and an important start on the history of technology from the point of view of those excluded by identity, disability, or circumstance from what the rest of the world might call progress.

KENNETH LIPARTITO

Dale H. Porter. *The Thames Embankment: Environment, Technology, and Society in Victorian London.* (Technology and the Environment.) xvi + 318 pp., frontis., illus., bibl., index. Akron, Ohio: University of Akron Press, 1998. \$49.95 (cloth); \$24.95 (paper).

Drawing on the history of technology, Dale Porter places the Thames Embankment in the context of London's uses of the Thames River, a force of nature. People build elaborate public works to improve natural amenities for urbanization, but these works alter nature. For example, London Bridge connected the city across the barrier of the broad Thames River, but its thick piers changed the flow of the river and thereby created scour, current, and stagnation problems. Porter frames the interactions among nature, society, and technology beautifully (Ch. 1), and the book fulfills in part the promise of a sustained historical analysis in this vein.

People build cities on rivers that carry away their sewage, for without such drainage human settlements would be awash in deadly wastes before growing into urban places. London's public works have revolved around water services associated with the Thames River: bridges, clean water provision, navigation, sanitation, and drainage. The Thames Embankment is one public work that has a long history of construction, multiple constituencies that influenced its form, and the simultaneous provision of many services. Because of these qualities, Porter has found it a fine focus for a study of interactions between society and technology. In addition to explaining the social construction of this technology, Porter details its physical construction as a public work. Readers interested in professions or a kind of historical sociology of public works will benefit from his descriptions and analyses of the personalities and effectiveness of commissioners, riparian landowners, key politicians, and major contractors and their family firms.

As it flows through London, the Thames River reacts to the influence of the tides, which prevent

any assertive flow that would bear away to the ocean the awesome load it carries. The capital itself sits on a broad subterranean expanse of "London clay," which supports foundations for great buildings but prevents the natural drainage of urban effluents from a large, dense city. Low tides exposed the riverbanks, and the river's many wharves and bridges reduced the flushing action of the tide, thereby permitting filthy water to stagnate and deposit its sewage load on vast mudflats at low tide. Porter vividly describes the unsavory fogs and stinks redolent of sulfur or ammonia that evaporated off these deposits (pp. 55–57, 129). The Thames Embankment, with its vertical shore, tidied docks, embedded intercepting sewers, conduits for other networked infrastructure, and roads, railroads, and promenades, embodied a grand solution to the original problems of pollution, navigation, and drainage. It even solved some emergent problems of the nineteenth-century metropolis, such as encouraging highly polluting industries to move downstream and making space for railroads.

The book includes one map of the urban Thames that names twenty-six landmarks (p. 27) but does not orient the reader adequately to the multiplicity of place-names mentioned in the text. The several chapters of public works history should interpret the Embankment's planning, management, financing, and construction in relation to urban ecology, the discipline Porter introduces (pp. 10–13, 75–76) as important to his analysis. Without this linkage, *The Thames Embankment* is an important volume in the history of technology because it analyzes this great public work very well and from many angles, but it does not realize the promise of explicitly joining this history of technology with human or urban ecology or with environmental history, although the author cites and clearly admires the contributions of colleagues who have written such works. As Porter joins Joel Tarr, I. G. Simmons, Martin Melosi, Christopher Hamlin, Bill Luckin, Anthony Wohl, and Lawrence Breeze in analyzing people and water, it is evident that soon a study of urban waters may synthesize these disciplines into a powerful method of historical analysis.

BETSY MENDELSON

Helmut Kettenmann; Jörg Zaun; Stefanie Korthals (Editors). *Unsichtbar—Sichtbar—Durchschaut: Das Mikroskop als Werkzeug des Lebenswissenschaftlers.* 93 pp., frontis., illus., bibl. Berlin: Museumpädagogischer Dienst, 2001. (Paper.)

This attractive, well-illustrated book was produced by the Deutsches Technikmuseum Berlin as part of a program entitled "Wissenschaft im Dialog" ["Science in Discourse"]. The objective of the program was to promote discussions about science between the general public and research institutions. For its part in the program, the Technikmuseum focused on the microscope as a means to elucidate the structure of living beings, restricting its coverage of this vast theme to events and developments in Berlin.

The book is divided into twelve chapters. Most of these deal with scientists, microscope makers, and microscopes in the nineteenth century. The remaining chapters, apart from an essay on the development of the electron microscope by Ernst Ruska, include a general introduction to the history of microscopy and treatments of modern techniques in light microscopy and modern microscope systems.

During the nineteenth century many instrument makers were at work in Berlin; the most important figures among these were Friedrich Wilhelm Schiek and Edmund Hartnack. Schiek's firm, in particular, receives a lot of attention in this book. One chapter discusses the troubles Johannes Müller and his pupils, who favored Schiek's workmanship, experienced in obtaining microscopes for their research. The excitement of the new prospects in microscopy opened by the concepts of the cell theory in the 1830s is underscored by the willingness of these dedicated scholars to pay no less than half a year's salary for a microscope. Fascinating, too, is the chapter on Rudolf Virchow, who came to Berlin in 1856 and brought with him from Würzburg, besides a plan for a new pathological laboratory, a *mikroskopische Eisenbahn*. This was a didactic appliance that consisted of two long tables with a rail fastened in between. Microscopes mounted on wheels were pushed along the rail from one student to the next.

This book was meant as a supplement to an exhibit in the Deutsches Technikmuseum Berlin that lasted for only a few days. It is the result of collaboration by a team of authors and is naturally aimed at the general public. The authors have produced a very informative, highly readable, and generously illustrated book. The essays offer admirable insight into various aspects of microscopical research (topics of research, microscope making and trading) in a period when Berlin was an important center in the scientific frontier that was microscopy. The essays are nicely balanced between the overly popular and the overly scholarly. Browsing through the illustrations in the book, I must say that, as a museum

curator, I would very much have liked to see the exhibit.

MARIAN FOURNIER

Pamela Thurschwell. *Literature, Technology, and Magical Thinking, 1880–1920*. Edited by **Gillian Beer**. (Cambridge Studies in Nineteenth-Century Literature and Culture, 32.) 194 pp., notes, bibl., index. Cambridge: Cambridge University Press, 2001. \$54.95 (cloth).

Despite the chronology implied by the dates in the title, this is not really a work of history of science in any usual sense. Rather, it falls under the rubric of cultural studies with a focus on literature, particularly that of and associated with Oscar Wilde and Henry James, and psychoanalysis, especially the strange theories of Sándor Ferenczi. The theme that ties the rather disparate components of this study together is what Pamela Thurschwell delineates as "shifting models of the permeability and suggestibility of the individual's mind and body" during the 1880s and 1890s, anxieties about which "erupt in crisis around sexuality" (p. 2), particularly homosexuality.

Contributing to these anxieties over the undermining of personal autonomy (mental and somatic) were the development of psychical research interests, particularly in telepathy, and of new "teletechnologies" such as the telegraph and the telephone. It should be stated at the outset that "technology," as such, is mainly deployed in this work as a source of analogies for psychical, literary, and psychoanalytical speculations.

The opening chapter, on the London Society for Psychical Research and its "experiments in intimacy" (thought transference or telepathy), sets the tone for the rest of the work. Thurschwell concentrates on a few leaders of the SPR, notably F. W. H. Myers, to relate their ideas and activities regarding telepathy to contemporaneous electrical teletechnology, to literature, and to notions of sexuality. In regard to this last theme: interest on the part of the SPR in studying "psychical" mediums (mediums who could effect physical changes during the séance) gave way to a focus on mental mediums. The study of the former, usually lower-class women, by the establishment male investigators of the SPR gave rise to an obvious set of potential sexual tensions and transgressions. Thurschwell argues that the shift to the study of mental mediums (who reproduced thoughts from the living or the dead) was meant in part to obviate these problems. But

study of thought transference was itself erotically charged—the eroticism of melding minds.

The middle three chapters are built around literary figures: Oscar Wilde (Ch. 2) and Henry James (Chs. 3 and 4). Although Thurschwell does not make anything of it, each of these chapters is focused on a particular word-concept: “influence,” “identification,” and “intimacy.” The case of Wilde and “influence” is particularly clear-cut and striking. “Influence” was related to suggestibility, to hypnosis, and to telepathy. In the 1890s, Thurschwell argues, anxieties over influence exerted by powerful, malevolent figures found copious literary expression—in Du Maurier’s *Trilby*, in Wilde’s *The Picture of Dorian Gray*, and in Wilde’s own trial, where his writings as well as his personal actions were held up to criminal scrutiny. Hypnosis, telepathy, and homosexuality all converged to produce a crisis of anxiety over “influence” in the 1890s.

The chapters on Henry James may not interest historians of science as much as the chapters on telepathy and psychoanalysis. However, the second of the Jamesian chapters (Ch. 4) does have some material that may prove illuminating to historians of science and technology. It deals with the deployment of teletechnologies like the telegraph (in James’s *In the Cage*) and the typewriter, usually by women. This latter teletechnology finds a focus in James’s longtime secretary and amanuensis, Theodora Bosanquet. Bosanquet was not only James’s favorite secretary (“a civilized typist,” in his words [p. 89]), but after his death she went on to her own literary destiny in the Bloomsbury Circle, writing a memoir on James, *Henry James at Work*, and, perhaps most interestingly, became an adept at the psychical skill of “automatic writing,” producing postmortem communications from James and other literary figures.

The keyword for this chapter is “intimacy.” This may seem strange for such a context, but Thurschwell’s point is that these deployers of teletechnologies are at once both unmediating “tools” and “mediums” who affect the information they are processing and transmitting and are brought into intimate relationships with their clients, the producers or recipients of this information. The changes in intimacy brought about by these new communication technologies impinge, naturally, on the contemporaneous crisis over changing sexual orientations and relationships. The extreme example of all of this is found in the automatic writing of Bosanquet. At one point, the “spirit guide,” Johannes, ordered Bosanquet to assume the vows of a nun (particularly chastity) in order to be the properly focused “in-

strument of a great work of wonderful and appealing beauty” (quoted on p. 104). Happily for Bosanquet, the spirits of both William and Henry James interceded to counter this demand. There is much deployment of literary theory and Lacanian psychology by Thurschwell in this chapter, which my own lack of expertise (and, in some cases, lack of comprehension) necessitates that I pass over in silence.

In the final chapter, “Freud, Ferenczi, and Psychoanalysis’s Telepathic Transferences,” themes of the earlier chapters are reconfigured in psychoanalytic sites. For example, Freud’s and Ferenczi’s attitudes and approaches to telepathy are explored over a broad range: from parallels between thought transference and psychoanalytic transference, to considerations of Lamarckian imprints on Freud’s thoughts about primal images, to anxieties about plagiarism. Indeed, in this chapter Thurschwell produces something of a synthesis of the entire study. Although Freud attempted (heroically) to maintain boundaries, Ferenczi dissolved distinctions between subject and observer, patient and doctor, reality and fantasy, psychosis and sanity, occult and normal (or scientific), homosexual and heterosexual norms. Here’s a sample: “Beginning with his theories about the developed hypersensitivity of the abused, Ferenczi speculates on the possibility that all hallucinations are really ‘an illusionary working through of real events.’ Invoking the always useful teletechnology analogy, he continues on to propose that dreams might be telepathically shared, the analyst can become simply a relay station between the thoughts of his patients as they communicate to and through each other” (p. 147).

To conclude, I return to my opening thought. If the core of a historical study is a temporally structured narrative, then this work falls outside of that genre. *Literature, Technology, and Magical Thinking, 1880–1920*, is a meditation on important and often overlooked aspects of the turn-of-the-twentieth-century *Zeitgeist*, centered on the understanding of personal intimacy, both somatic and intellectual. The book was a difficult read for this traditional historian, but one with a good deal of value.

SEYMOUR MAUSKOPF

Paul K. Alkon. *Science Fiction before 1900: Imagination Discovers Technology.* (Genres in Context.) xx + 177 pp., illus., bibl., index. 1994. New York/London: Routledge, 2002. \$18.95 (paper).

This volume provides us with an informed and compact overview of the literary genre of sci-

ence fiction before 1900. More than just an introduction, Paul Alkon's book is a model of critical writing about a genre. Beginning with the history and aesthetics of science fiction, he locates its major themes, its key social functions and concerns, and its narrative strategies before turning his attention to British, French, and American writers, concentrating on a few exemplary works that mark the most significant phases in the early evolution of science fiction. Among these are such well-known texts as Mary Shelley's *Frankenstein* (1818), H. G. Wells's *The Time Machine* (1895), Jules Verne's *Twenty Thousand Leagues under the Sea* (1870), and Edward Bellamy's *Looking Backward* (1888); less familiar works such as Albert Robida's *Le vingtième siècle* (1883) and Auguste de Villiers de l'Isle-Adam's *Tomorrow's Eve* (1886); and works not immediately associated with science fiction such as Mark Twain's *A Connecticut Yankee in King Arthur's Court* (1889) and Jonathan Swift's *Gulliver's Travels* (1726). The book concludes with an excellent bibliographic essay on key reference works, historical and theoretical studies, and works on individual texts and writers. Alkon knows his subject thoroughly and writes about it in a lively and accessible style. His critical analyses of key texts are invariably insightful and stimulating, motivating us to read (or reread) the works under discussion.

Surveying numerous efforts to define this disputed and often maligned genre, Alkon draws useful distinctions between science fiction, utopia, fantasy, myth, satire, and other forms. What distinguishes science fiction from other narratives labeled "speculative fiction" or "structural fabulation," he maintains, is that it is first and foremost a literature of change and transformation. Secular in its outlook on modern science, it "excels at articulating the new possibilities for good and evil that shape our destinies in an age when science has accelerated the proliferation of technologies once beyond even the reach of fantasy" (p. xi). For Alkon, the genre's impetus as well as its overriding master theme is its representation of the impact of science and technology on social life. Reworking key metaphors circulating in the world of its composition, science fiction achieves its full effect through the key strategy of "cognitive estrangement" or defamiliarization (pp. 10–11, 34, 67). Essentially the idea here is that in projecting fictive alternatives into an imaginary space or time, science fiction critically distances our own present moment, transforming it into a determinate past and making it more accessible to critical understanding. Seen this way, the real function of science fiction

is not to project viable futures or prophecies of things to come but to set off the present as a moment in history and therefore as transitory and capable of transformation. In our chaotic, fragmented, and bewildering postmodern world, this is surely the most important social function we can ascribe to this genre.

Moreover, "since *Homo technicus* is also *Homo ludens*," Alkon also finds that science fiction writing is distinctly self-conscious and self-referential, inviting readers to participate playfully in producing meanings (p. 136). The best science fiction, he argues, provokes intense responses from readers and stimulates our engagement with the ambiguous cultural meanings of technology.

Of course, by focusing exclusively on the best that this genre has to offer, Alkon leaves out much that is trivial, vacuous, reductive, and xenophobic about it. But he also demonstrates effectively why science fiction seems to have become the supreme expression of our cultural moment—and why this genre is especially suited to narrate and cognitively map the complexities of our technological culture.

PETER RUPPERT

Rebecca Bedell. *The Anatomy of Nature: Geology and American Landscape Painting, 1825–1875*. xiv + 186 pp., frontis., illus., bibl., index. Princeton, N.J./Oxford: Princeton University Press, 2001. \$45.

When last I visited the National Museum of American Art in Washington, D.C., I listened astounded as a senior curator accused Thomas Moran of inciting Americans to be expansionists. We should ignore Moran's reputation as a founding artist of the national parks, this curator seemed to say; rather, the point of his use of yellow to portray the Grand Canyon of the Yellowstone was to announce the presence of GOLD! in the American West. "Come and get it," Moran was saying. Bluntly, the grandeur of his paintings was subliminally a commercial ruse.

To Rebecca Bedell's credit, this book corrects that absurdity. It is hardly a fault in any artist to paint the truth of what exists. After all, the Grand Canyon of the Yellowstone is yellow, and in many moods of weather and light it can be gold. The composition of a painting is another matter; artists have always taken license there. But in doing so, Bedell argues, many in the nineteenth century were guided by science as much as by preference. Besides Moran, she devotes chapters to Thomas Cole, Asher Durand, Frederic

Church, John Kensett, and William Stanley Haseltine. Each was uniquely committed to geology as a basis for understanding landscape. More than a passing interest, geology was the passion of artists concerned about portraying the landscape credibly.

However, just because a few writers have advanced an absurdity—nineteenth-century artists as gold diggers, and sometimes worse—are those scholars now recommitted to the truth somehow entitled to claim the field? In Bedell's case, she principally quotes her peers. Even if her argument is that previous scholars are out of date, how and why that is so must be explained. Meanwhile, her "Selected Bibliography" is a poor excuse for selecting other scholars out. Where, for example, are Henry Nash Smith and Leo Marx, Hans Huth and Roderick Nash? Where are James Thomas Flexner and Elizabeth McKinsey? They, too, talk about the artists of the nineteenth century in terms of an American identity. Even if geology were the greater part of that identity, ignoring the bibliography to advance one's case for originality is like accusing Thomas Moran of inciting a gold rush.

Art is many things, and never a simple thing, which is what invites a constancy of reinterpretation. As it stands, there were many trends, advances, and emotions in the nineteenth century that help explain its artists' motives. To be sure, Bedell succumbs to the temptation to wander away from geology and cover some of those other popular bases too. That is fine; but again, whenever the wandering is not entirely original, full citation would seem appropriate.

Because *The Anatomy of Nature* takes the high road, its faults are easy to overlook. It is well written, creatively organized, and beautifully produced. It is as fine a point of departure for studying the subject as any book in the field. Geologists will love it as a celebration of how their science has influenced the world of art. Not to stop with this book is still the point. These artists are new neither to history nor to historians of the American experience. Scholars who covered the subject first were no less committed to covering it well.

ALFRED RUNTE

Peter J. Bowler. *Reconciling Science and Religion: The Debate in Early Twentieth-Century Britain.* (Science and Its Conceptual Foundations.) xiii + 479 pp., illus., notes, bibl., index. Chicago: University of Chicago Press, 2001.

The last couple of decades have witnessed a phenomenal growth in histories about science and

religion, and with it significant developments in historiography. Historians have been ardently attacking simple, universal models such as the "warfare thesis" and the "harmony model." In recent works, science and religion are shown to be complex, multifaceted phenomena, and the interactions between them are explored in great specificity. The particulars have come to be seen as the key to understanding the varieties and differences that abound in this flourishing field. Peter Bowler's excellent intellectual history *Reconciling Science and Religion: The Debate in Early Twentieth-Century Britain* stands as an important contribution for just this reason.

Bowler has done a fine job of bringing together different strands of the British intellectual world in the late nineteenth and early twentieth centuries. He avoids conceptualizing the science-religion discussion in monolithic terms but does not throw up his hands and tell us merely that the situation was complicated and leave it at that. Rather, he amasses an impressive number of primary source readings to paint a vivid picture of the period's tendencies and countertendencies. He demonstrates the importance of disciplinary affiliation to a person's perspective, how generational differences affected worldview, and the ways that intellectual movements interacted with each other—sometimes with such a long lag time that outdated ideas in one field came to be seen as demonstrable proof in another. There are fascinating episodes to be found here, and the careful reader leaves the book with the insight that Aha! one can make sense of it despite the complexity.

The title *Reconciling Science and Religion* is actually somewhat misleading because it suggests a monolithic thesis that Bowler's study abjures. Bowler explains his project more accurately on the first page of the book, characterizing it as a study in the rise and decline of a reconciling attitude. But even that is too simplistic given all that Bowler does with his sources. The rise and decline thesis does, however, help correct a widespread error that depicts religion in steep decline after Darwin and that sees the British intelligentsia at the end of the nineteenth century as having pretty much given up on it for good. Books such as A. N. Wilson's popular *God's Funeral* (Norton, 1999) have tended to reinforce the impression that after Darwin it was all boxing and bruising as science gradually superseded religion as a source of authority. Bowler shows this picture to be false by uncovering a wealth of material that historians of science have largely ignored.

This long, dense book explores both the di-

versity of intellectual traditions current in early twentieth-century Britain and the transformations taking place in those traditions over the course of about three decades. Some of what Bowler tells us fits a standard secularization thesis very well. Religious institutions were in decline, their influence waning considerably among the intelligentsia. Individual scientific disciplines were becoming professionalized and institutionalized. Younger scientists, those socialized after the turn of the century, by and large found it unprofessional—even embarrassing—to invoke God or speculate broadly about theological matters in public.

These secularizing tendencies existed, Bowler shows, but not exclusively. The decline in institutional religious influence was neither total nor entirely steady. There was a surge in religious affiliation after World War I and a general conservative turn toward traditional religion in the later postwar years. Furthermore, institutional affiliation doesn't tell the whole story: independent thinkers often expressed an interest in spirituality even as traditional religion lost cachet. Nor should we be too quick to generalize about the effect of professionalization on the sciences. Older scientists such as Arthur Eddington and James Jeans did not feel the same resistance to talking about theology that their younger colleagues did. Quite the contrary. They believed it almost an obligation to extend and develop what they saw as the theological ramifications of their work, following the older and much-respected natural theology tradition. With prestige and loud voices, these older scientists' views came to be seen outside of science as more representative of scientific orthodoxy than they actually were.

Bowler shows also that materialism, that great curse of the Victorian period—which we all know gave men headaches, insomnia, and suicidal urges—met a shifting fate. The standard image is of course exaggerated and obscures contrary strands of thought such as the implications of late nineteenth-century ether theory in physics. Also, by the turn of the century several formidable defenses against materialism arose. Most important, idealism regained its intellectual stature, advanced in no small part by Henri Bergson's idea of a life force in matter, an *élan vital*. Further, Darwinism had few supporters among evolutionists at the end of the century, and many biologists adopted nonmechanistic models of development. In this vein, C. Lloyd Morgan's notion of emergent evolution became popular since it left room for purpose in nature. Slightly later, the new physics gave birth to the

notion of quantum indeterminacy, which also suggested a natural world open to nonmaterial causation. All in all, there was a significant assault on materialism from both outside and inside science that gave arguments for religion a great boost.

Among Bowler's most important contributions is the attention he pays to disciplinary distinctions. What was true in biology was not the case in physics. In the 1920s biology took a sharp turn back toward materialism and determinism with the Huxley-Fisher-Haldane synthesis of genetics and natural selection, a move that robbed biology of its easy support for teleological and mind-centered views of the universe. At the same time, a reverse movement took place in physics as the quantum revolution began sowing a fertile field for those seeking the reintroduction of God and spirit into the material world. Of course, even in biology there was not a single direction. In two fascinating passages, Bowler explains how two of the architects of the synthesis, Julian Huxley and R. A. Fisher, found ways of salvaging some of that earlier vision (albeit quite differently) and hence retaining a religious vision.

When one begins looking at the way ideas were shared between disciplines the story becomes even more interesting. Among religionists and professional writers who were nonscientists, the latest developments in science were not always noticed—let alone adopted—so conclusions drawn from science often tended to be out of date. Outside of biology, for instance, the decline of idealism and teleology was hardly noticed. *Élan vital* and emergent evolution continued to be widely touted as current science even as the new synthesis became ensconced among biologists.

Bowler's sensitivity to the differences between the scientific disciplines is admirably matched by his breadth and thoroughness in discussing the diversity of religious attitudes toward the sciences. He considers the ideas of a panoply of religionists and irreligionists alike: Anglican bishops, Roman Catholic theologians, Free Church ministers, nonaffiliated religionists, spiritualists, atheists, agnostics, and rationalists. In the religious mainstream, the reconciling attitude was most prevalent among Modernists. These were the folks who believed that religion ought to be saved by making it acceptable to modern knowledge and contemporary science. It is not surprising to find that when religious Modernism fell into disrepute in the 1930s with the rise of Neo-orthodoxy, so too did the efforts at finding ways to adapt religion to science. Unlike

in America, where Fundamentalist opposition to modern science played a significant role, in Britain the most vocal conservatives merely ignored science. For these religionists science was irrelevant to theology, not necessarily opposed to it. But, as Bowler points out, these conservatives didn't make their impact until the 1930s, and by that time most of the professionalized younger generation of scientists were just as assiduously avoiding theology. Bowler ends his study by noting that professionals in both science and religion expressed aversion to the reconciling program that had been so popular earlier in the century. But even this has been a temporary phenomenon; looking ahead, he shows that there have been subsequent waves in which efforts at reconciliation once again rose to prominence. Indeed, we find ourselves in one such period today.

On the whole, this book significantly advances our understanding of this complex period, introducing new material and synthesizing well-known events in a convincing framework. Bowler surveys a vast literature—his bibliography runs to over forty pages and nearly a thousand entries—and brings together items as diverse as popular books and novels, articles in scientific and religious periodicals, and published radio broadcasts. I predict that *Reconciling Science and Religion* will be heavily used by intellectual historians as well as historians of science in understanding religion and science issues in modern Britain.

STEPHEN P. WELDON

■ Recent (1950–)

Antony Milne. *Doomsday: The Science of Catastrophic Events*. xii + 194 pp., bibl., index. Westport, Conn.: Praeger, 2000. \$55.

This book aims to provide a concise review of the study of catastrophic events. Its scope is ambitious: it ranges from the Big Bang to the Big Crunch; more locally, it considers the effects on our earth of volcanoes, floods, earthquakes, impacts from comets or asteroids, climatic change, and disease. Antony Milne has read widely, spoken to diverse experts, and gleaned an impressive amount of information. We meet archaeology, astronomy, atomic physics, chaos theory, geology, molecular biology, and theories of evolution. Myths and legends from ancient times are matched both across continents and to modern data and theories.

I have good knowledge in only a few of the areas explored, but I found that several statements concerning numerical quantities need

qualification. We are told that the moon's gravitational influence on earth is greater than the sun's; but although the moon's influence on *tides* exceeds that of the sun, the ratio of *total* gravitational forces is about 180:1 in favor of the sun. Kilotons and megatons are confused in comparisons with the size of the Hiroshima atom bomb. Two events believed rare are accorded the chances of "million to one" and "billions to one," with no justification of either figure. The assertion that "if some rare disease rears its head about 12 times a year, this does not mean a case of one a month. Spread out over time, the disease would be seen on average just once every 164,000 years" (p. 158) simply puzzles me. On other numerical matters, it would have been better to present the data on volcano eruptions and earthquakes in tabular form, rather than among prose passages, to assess their possible periodicity.

At times, surprising assertions are made without supporting evidence. For example: "Jupiter has . . . deflected out of our orbital path those destructive bolides and meteorites that would have periodically destroyed all life on Earth" (p. 102); and "Probably, for the first time in the universe, it (the Moon) enabled organic life to take hold on one planet" (p. 50). It is said that Halley was the first European actually to see the comet that bears his name. If so, why is it shown on the Bayeux tapestry? Some "evidence" offered is unconvincing: Is it possible that early historical sagas could provide evidence of changes in the lengths of the day, and the year, *since 200 million years ago*?

Nevertheless, the book gives many timely warnings, with appropriate evidence. Consideration of the possible global economic effects of a major earthquake in Japan will lead readers to thoughts of the 9/11 atrocities in the United States and the delicate balance on which Western prosperity rests. Milne points out how expert opinion has frequently changed radically in response to new discoveries. Noting earth's large temperature fluctuations in the fairly recent past, he cautions against leaping to conclusions about global warming. Works to control floods have encouraged settlements in inadvisable locations. We are reminded to take a proper perspective on the relative risks of adverse events that might affect us, despite the manner of their reporting by a sensationalist press. The overreaction of the British government to the risks posed by BSE (mad cow disease) is rightly castigated. But even in a book dominated by catastrophes, "Let us hope that the third millennium will not be as bad as the second" (p. ix) is surely too gloomy: how

many of us would exchange our present lot for that of those alive a thousand years ago?

JOHN HAIGH

American Association for the Advancement of Science. *Blueprints for Reform: Science, Mathematics, and Technology Education.* (Project 2061.) xviii + 300 pp., illus., figs., app., bibls., index. New York: Oxford University Press, 1998. \$17.95 (paper). Also available on the website <http://project2061.aas.org/>.

Most historians of science may see little reason to preoccupy themselves with the problems of primary and secondary education in the United States—unless they experience at first hand the dismal state of a specific school to which they are sending their children. At that point they might agree with the well-known paragraph in the first page of the report *A Nation at Risk*, issued in 1983 by the National Commission on Excellence in Education: “If an unfriendly foreign power had attempted to impose on America the mediocre educational performance that exists today, we might well have viewed it as an act of war. . . . We have, in effect, been committing an act of unthinking, unilateral educational disarmament.”

To be sure, in the meantime there have been some signs of improvement, many of them instigated by governors as part of their efforts to make their states more attractive to industry through improvement of the workforce. Among other notable efforts in that direction, two mutually supporting ones stand out: a report of the National Research Council of the National Academies of Sciences (NAS) and the fruits of the ten-year-long Project 2061 of the American Association for the Advancement of Science (AAAS). The NAS product, released in December 1996 under the title *National Science Education Standards* (Washington, D.C.: National Academy Press, Code SCISTA), should be of great interest to historians of science. For it states unequivocally: “In learning science, students need to understand that science reflects its history and is an ongoing, changing enterprise. The Standards for the history and nature of science recommend the use of history of science in school science programs, to clarify different aspects of scientific inquiry, the human aspects of science, and the role science has played in the development of various cultures” (*National Science Education Standards*, p. 107). Indeed, the volume then shows in detail how this might be done, grade by grade, science by science.

The AAAS Project 2061, after lengthy re-

search by large teams of educators and scientists, released two thoughtful and detailed reports on improving secondary education: *Science for All Americans* (AAAS, 1989) and *Benchmarks for Science Literacy* (AAAS, 1993). These also stressed the need for a historical component in science education.

Turning now to the most recent result of AAAS’s Project 2061 (that number indicating a date—and a mixture of hope and despair that at the next return of comet Halley all will be well): *Blueprints for Reform* is once more the result of a long study, supervised by F. James Rutherford, Gerald Kulm, and Andrew Ahlgren. It attempts to synthesize ideas from over three hundred consultants—individuals in education, the sciences, technology, and industry—as well as a large number of institutional contacts. There is also evidence, not least in the huge bibliographical apparatus and statistical charts, of the AAAS staff’s own research.

The fundamental aim of the book is to help would-be reformers “who are engaged and thinking about systemic reform in science, mathematics, and technology education” (p. vii). The key idea here is the recently fashionable concept of studying first the strengths and failures of the interacting parts within the national educational system, rather than trying first to mend problems in this or that portion of the whole. In the same spirit, the support-for-education division of the National Science Foundation, now blessed with an annual budget of some \$800 million, has put heavy emphasis on “systemic reform” in the United States, the country that, as even Tocqueville marveled, has no central guiding authority such as other countries’ ministries of education.

The “system” is of course difficult to discern among this vast continent’s ever-changing, poorly connected parts—students, teachers, administrators, PTAs, curriculum developers, publishers, professional societies, test organizations, employers, schools of education, government agencies, and so forth. There is not necessarily any similarity even between the curricula in schools of two neighboring parts of one town; and both generally will not even try to correct the pervasive upside-down regime of topics taught but will continue to offer, contrary to the need to understand each properly, first biology for the many, then chemistry for the fewer, and finally physics for the smallest remnant. Aware of these conditions, *Blueprints* nevertheless tries to identify those components’ boundaries and possible interactions of the educational systems “that matter most in thinking about the attainment of science literacy by all K–12 graduates.”

This effort has resulted here in the identification and description of twelve aspects, pervasive despite all local differences. They are laid out in chapters given in alphabetical order: "Assessment," "Business and Industry," "Curriculum Connections," "Equity," "Family and Community," "Finance," "Higher Education," "Materials and Technology," "Policy," "Research," "School Organization," "Teacher Education." Historians of science will be glad to find, on many pages, eloquent arguments to connect science teaching with the humanities and to strive for interdisciplinarity in the curriculum. The report even has the courage to point a finger at specific, well-known national reform movements that fail in this respect (see, e.g., pp. 124–125).

Even though it is a gold mine of ideas and references for reformers and students of education, the report is modest in self-assessment, with remarks such as "This is at best . . . only a first step" and pleas to have readers respond by e-mail to such questions as "Are there some themes that could be used to bring greater coherence to the collection?" And elsewhere, "The job is not over." Indeed; but this volume is a good deal more than a first step toward educational rearmament.

GERALD HOLTON

Kurt Vogel; A. P. Iuskevich. *Mathematike-geschichte ohne Grenzen: Die Korrespondenz zwischen K. Vogel und A. P. Juschkewitsch*. Edited by **M. Folkerts, M. M. Rozanskaja, and I. Luther**. (Algorismus: Studien zur Geschichte der Mathematik und der Naturwissenschaften, 22.) xxxv + 263 pp., notes, index. Munich: Institute für Geschichte der Naturwissenschaften, 1997. DM 29.80 (paper).

A. P. Youshkevich; K. Vogel. *A. P. Youshkevich–K. Vogel: Istorija matematiki bez granic*. Edited by **M. M. Rozanskaja, I. Luther, and M. Folkerts**. 310 pp. Moscow: Janus-K, 1997.

In 1997 a remarkable volume was published simultaneously in Moscow and Munich: the correspondence between the Russian-Soviet historian of mathematics Adol'f Pavlovich Youshkevich (1906–1993), the "grand seigneur" of Soviet historians of mathematics, and the leader of German historians of mathematics, Kurt Vogel (1888–1985). Their correspondence had begun in 1955, when official diplomatic relationships between the Federal Republic of Germany and the Soviet Union were established, and continued until Vogel's death in 1985. For thirty years the two historians remained in con-

tact, despite the political and social changes in their two countries and the concomitant political and ideological conflicts, debates, and discussions. Not only is the correspondence itself remarkable, but the two participants led quite different lives and had to overcome very different conditions to become leading figures among the historians of mathematics in their respective countries.

Kurt Vogel studied mathematics and physics at the universities of Erlangen and Göttingen. From 1913 until 1920 he served as an officer in the Imperial Army, and after the war he began to teach, first at a *Realschule*, then from 1927 to 1954 as a professor at a *Gymnasium* in Munich. In 1936 he became a *Privatdozent* at the university in Munich and the head of the Institute for History of Science and Mathematics. His first interest was the history of Byzantine mathematics, but he later studied the history of Babylonian mathematics as well. He undertook a new edition of Johannes Tropicke's important *History of Elementary Mathematics* and also produced editions of several early mathematical works, including the *Bamberger Rechenbuch* of 1482. At the same time he studied the history of Greek mathematics, especially arithmetic. Despite officially retiring in 1963, he continued to work at the university until 1970. His honors included the Sartori Medal of the History of Science Society, which he received in 1969, and he was a member of the oldest academy of science in Germany, the Leopoldina Halle (Deutsche Akademie der Naturforscher Leopoldina).

Adol'f Pavlovich Youshkevich was born in Odessa. Thanks to Russian imperial politics he was brought up among anti-Semites. He was educated at schools in St. Petersburg, which became Petrograd after 1914, then studied mathematics at Moscow University and became a mathematician. Between 1930 and 1952 he worked at the Technical University in Moscow, from 1940 as a professor of mathematics. During World War II he was evacuated from Moscow with other members of the university. He survived Stalinism more or less unscathed, but he never forgot the years of the "great terror," 1936–1938, and the anti-Semitic crackdowns against so-called cosmopolitanism in the 1940s. In 1945 he got the chance to work in the Institute for History of Science and Technology at the Soviet Academy of Science (recently renamed the Vavilov Institute of the Russian Academy of Science) in Moscow. In 1952, when he was expelled from the Technical University because of anti-Semitism, he continued to work in the institute, becoming head of the Department of His-

tory of Mathematics in 1960. He held this post until his death in 1993, through all the changes both in the country and in the Academy of Science itself. He became a specialist in various fields of the history of mathematics, especially the history of analysis and the work of Leonhard Euler. He wrote several books and brought out many edited works on the history of mathematics. He too was awarded the Sarton Medal and was a member (1958) of the Leopoldina Halle. Everyone who had the good fortune to know him personally was impressed by his enormous knowledge, his charm, and his humor—and by his behavior as the “German professor,” his nickname at the institute.

These very different historians of mathematics, both of whom were interested in ancient Babylonian, Greek, and medieval mathematics, knew each other chiefly through their work. Vogel was one of the rare historians of mathematics in the Western world who could read and speak Russian. When correspondence with colleagues in the “capitalist countries” of the West was permitted to those in the institutes of the Academy of Science (under strict restrictions, as the introduction reveals [pp. xxxi–xxxii]), Youshkevich wrote to Vogel, marking the beginning of their long correspondence. Because of political circumstances in the Soviet Union, travel was difficult for Youshkevich. Meetings between the two correspondents were thus very rare. Their first encounter took place in East Berlin in March 1957, at the celebration of the 250th birthday of Euler. Other meetings came about at the International Congresses for History of Science in Ithaca (1962), Paris (1968), and Moscow (1971).

The reader of these letters must bear in mind the particular political conditions under which both participants wrote: Kurt Vogel in West Germany, a free country tainted by hysteria with regard to the “Reds” and the Soviet Union; Adol’f Youshkevich in the post-Stalinist Soviet Union, where the activities of all inhabitants and especially their relations with the West were strictly policed and censored by the KGB. But they were historians of mathematics, and their letters must have been dull reading for their censors. Poor censors; Youshkevich and Vogel no doubt baffled them with their endless inquiries about the best translation for particular mathematical terms, the most appropriate definition of a function, and so on.

This dialogue between two founders of national schools of history of mathematics is interesting, as any letters between important figures in science or culture would be. But more than that, the correspondence allows us to learn about

the problems of history of mathematics as a field, both in terms of particular questions and in terms of methodological problems; it also reveals the difficulties of teaching the history of mathematics at the university level in the two countries. Both Vogel and Youshkevich were important in introducing lectures on the history of mathematics to the university system of their native countries. The reader will find many interesting subjects in this work. Moreover, the correspondence can help us to understand the world of science through the lives of two remarkable scientists who were important for their countries and who belonged to the old—horrible—twentieth century.

ANNETTE VOGT

Robert Royal. *The Virgin and the Dynamo: Use and Abuse of Religion in Environmental Debates.* xi + 271 pp., figs., index. Grand Rapids, Mich.: William B. Eerdmans Publishing Company, 1999. \$25 (paper).

Taking his cue from a perceptive metaphor of Henry Adams, Robert Royal envisages much of our culture as a conflict between “the Virgin” and “the Dynamo,” the former being a spirituality that encompasses beauty, human values, and religious belief, the latter essentially the achievements of modern science and technology. Elaboration of this dichotomy takes up much of the book, not least because of the ambiguities that lie in every one of the words used to describe it. Greatest of all the problems is the fact that religion has been used to justify both the exploitation of nature and its conservation. And of course the debates have long been haunted by facile generalizations, discredited predictions, flawed analyses, and much else that prompted the *cri de coeur*, “If we really want to know what we are about at our present moment, we need some very large intellectual perspectives” (p. 60).

Royal queries the common religious view that environmental disasters “are all part of one interlocking apocalypse” and considers fears of a population explosion to be “grossly exaggerated” (p. 19). He tends to be optimistic about the future effects of acid rain and does not consider ozone depletion in the upper atmosphere a potentially serious problem. Controversially, he views sustainable development as “a socialist mistake of thinking that a central bureaucracy can plan for the operation of a whole economic order better than the innovators and entrepreneurs within it” (p. 17).

In discussing the relevance or otherwise of re-

ligion in environmental discourse, Royal focuses on the recent past. The views of Thomas Berry and Brian Swimme are presented as a quasi-religious attempt to derive values from science itself. Humanity is seen as “the affliction of the world” (p. 119); waste is anathema; the ideal civilization would be that of a Neolithic village. Science and technology, as now practiced, are leading us further away from nature. In contrast, the poet Frederick Turner sees them as guiding us to a newer and richer conception of the great chain of being. He denies the strong claims of social constructionism, the inevitable harm of a greenhouse effect, and the transcendence of God over nature. These writers do not pretend to start from any biblical theology; and on the question of whether the Bible is for or against human “dominion” over nature, Royal points out that in different places it is both, so mature theological analysis is necessary to resolve the apparent difficulty.

A chapter on “deep ecology” is a useful guide to some of the movement’s leading figures, particularly its founder, the Norwegian philosopher Arne Naess. The phenomena of what Royal calls “ecofascism” and the “ecowarrior” mentality are presented as serious threats to a sane solution to ecological problems, with “ecotage” (or ecological sabotage) as a conspicuous weapon. Royal describes the radical ecocentricism of much deep ecology as “a cosmic muddling along” (p. 159) whose sweeping claims have yet to be tested over long periods of time.

The “creation spirituality” of Matthew Fox, characterized by its distinctive pantheism, is seen as vague synthesis of a large number of disparate if not dissident elements in our Western culture, from deep ecologists to animal liberationists. At their “watering hole” these groups lose much of their distinctiveness in a search for a common mysticism. Fox is criticized for his weak grasp of current environmental issues (as he might be on scientific grounds: he apparently believes the earth’s atmosphere to consist of oxygen, hydrogen, and ozone). The phenomenon of ecofeminism, though criticized for its internal contradictions and its unflattering portrait of men, receives a careful analysis, and there is a sensible discussion of writings by Carol Christ, Marti Kheel, Rosemary Ruether, and others. The book concludes with a short account of the impact of liberation theology on ecology and an appreciation of the environmentalism of Al Gore and Thomas Derr.

The book’s biggest problem is the lack of a clearly defined framework within which ideas can be discussed and evaluated. It seems that the

author for the most part inclines to a traditional view of science and history that owes much to the Bible, but his theological stance is not altogether clear. The alternative is a genuinely historical approach in which, in the mode of modern history of science, current phenomena are traced back to their origins where possible. For readers of *Isis* the absence of such an approach is probably the book’s most serious defect. Newtonianism is often mentioned as the start of our troubles, but little is said to connect seventeenth-century science with modern environmentalism. The famous line attributed to Laplace about God (“no need of that hypothesis”) is presented as fact rather than myth. Lynn White’s essay of 1967—a historical thesis if ever there was one—is passed by with the remark that “it has been challenged or qualified” in the last thirty years (p. 35). The ancestry of deep ecology is often said to include such diverse figures as St. Francis, the eighteenth-century Romantics, George Orwell, and many others—without the least indication of how they might have been linked.

We are thus presented with a survey of a vast range of ideas and, even more, of feelings, for the rational elements in much of current environmental thinking are often subsumed in and submerged by emotional New Age mysticism. Royal’s easy style makes helpful reading for a newcomer, and his penetrating critiques provide a dire warning to those who would too easily write about the environment.

COLIN A. RUSSELL

Robert G. Fleagle. *Eyewitness: Evolution of the Atmospheric Sciences*. ix + 129 pp., glossary, refs., index. Boston: American Meteorological Society, 2001. \$70 (cloth).

The first word in the title of Robert G. Fleagle’s memoir—“eyewitness”—sums up the book’s approach. He gives an eyewitness account of some of the major historical events within the atmospheric sciences in mid-twentieth-century America, but, alas, without placing them in a broader context or explaining why they were truly evolutionary.

Fleagle embarked on atmospheric sciences research in 1943 as one of thousands of Army Air Force cadets receiving graduate training in order to provide military weather support during World War II. From this beginning, he describes the unfolding of the many changes that have occurred since in the practice of meteorology and related sciences. He does so from his vantage point as a professor at the University of Washington and as a participant in numerous state,

national, and international organizations—scientific and governmental—that have addressed atmospheric policy issues.

The strengths and weaknesses of this book result from the memoir genre itself. Fleagle writes from his own experience—counting on his memories, backed up by his personal papers, and sparked by an oral history interview he gave to fellow scientist Earl Droessler in 1993. One finds some tantalizing bits here: that atomic physics was one of the required fields for his Ph.D. in physics-meteorology (1948) at New York University; that the increase in federal support for university research led to the demise of the University of Washington's Research Society; that the name of a prominent private-sector meteorologist (Irving P. Krick) who resigned from the American Meteorological Society owing to an alleged breach of the Code of Ethics was not mentioned in the society's council minutes; that while working in the White House Office of Science and Technology, Fleagle recommended that NASA take the leading role in meteorological research; about his frustrations with promoting interdisciplinary work to address environmental issues. Unfortunately, none of these points is followed up. By answering the question "Why was that?" in detail for each of these statements, and many more, this book could have made a much greater contribution to the history of science. Instead, it just provides recollections—some of which are preceded by the modifiers "maybe" or "perhaps" or "I think"—that are unusable without references to supporting documentation.

Fundamentally, the book suffers from offering vignettes about important events in meteorology by a meteorologist writing for other meteorologists. Fleagle uses technical terminology and explanations that will be indecipherable to anyone without a strong background in the field (the glossary is simply a list of acronyms); includes lists of names of coworkers, their graduate students, and the scientific awards they have received over the last fifty years; and makes "insider" comments that likely will make sense only to people long associated with the atmospheric sciences community. The book is much like a do-it-yourself *Festschrift*—long on laudatory comments and short on critical analyses of truly significant historical events.

Topic coverage is very brief. There are fifteen chapters in 110 pages of text. One chapter—on the huge Global Weather Experiment that began in the late 1960s—is only two pages long. Most index entries are treated on only one page.

For Fleagle's memoir to make a significant contribution to the history of science, it would

need to provide insight into the events he describes. Historians of the atmospheric sciences already know the basic outline of important events in the past sixty years. It would be nice to know what the participants were thinking and why. This pricey book isn't the place to look.

KRISTINE C. HARPER

Donald A. Beattie. *Taking Science to the Moon: Lunar Experiments and the Apollo Program.* xv + 336 pp., figs., illus., notes, index. Baltimore: Johns Hopkins University Press, 2001. \$42.50 (cloth).

Originally an exploration geologist working in the jungles of Colombia for Mobil Oil, Donald Beattie braved NASA's jungle from 1963 to 1973, the entire period of planning and execution for the Apollo lunar missions. The planetary geologist Don Wilhelms described lunar science from the U.S. Geological Survey perspective in *To a Rocky Moon* (Arizona, 1993); Beattie is uniquely situated as a former NASA scientist-manager to tell the space agency's side of the story. He reveals the meticulous planning needed to select experiments the astronauts could safely and efficiently conduct, the internal disagreements (and occasions of fruitful cooperation) among NASA centers and with contractors, and what might have been had the United States not abandoned the moon after *Apollo 17*. The book thus fills several gaps in the Apollo literature: details about particularly challenging experiments, contractor relationships, dissent within the seemingly monolithic NASA, and the many underappreciated successes of the program. It is part of the new series in NASA history, and once again NASA Chief Historian Roger Launius is to be commended for encouraging scholarship that shows the inner workings of his agency—warts, wonders, and all.

Although Beattie's target audience is readers who already know the basics of NASA history, there is much here that will be new to them. The writing is clear, too, and he puts readers on the moon with the astronauts, taking photos of and logging every specimen, getting contingency samples in case of emergency, and wrestling with uncooperative gear. He describes the precision with which every footfall was planned and mapped and the early confusion over some experimental readings (e.g., "moonquakes" caused by astronauts setting down backpacks). He drops tidbits suitable for a thesis or book, such as the scheme to construct cargo lunar excursion modules to build lunar bases.

Taxpayers can never be reminded enough that

every penny for space exploration is spent on earth, and Beattie details one illustrative example: Martin Marietta and Black and Decker developing the means to take eight-foot core samples without water to cool the drill bit. It is unimaginable that these companies did not immediately apply this technology to uses on earth, paying back citizens a thousandfold.

Beattie has his opinions and he expresses them. Lunar Orbiter was the best-managed project of the period, the astronauts did a fine job as geologists, the USGS did a B+ job at mapping, contractors really stepped up to the plate, NASA Houston was not a team player. He favors manned space exploration and a return to the moon and believes we never should have stopped going there. He doesn't proselytize, whine, or moan, however; he pitches no pie-in-the-sky schemes for commercial exploitation. He offers only a hint of the tremendous frustration and sense of loss—even betrayal—that contractors and NASA employees felt over America's failure to continue any sort of manned lunar and planetary exploration program.

In doing so, Beattie inadvertently hints at why space is still such a hard sell.

He describes meek attempts by NASA geologists to sell their ideas—they didn't politick, persuade, or arm-twist, just showed slides—revealing the too-typical assumption that Science and Knowledge are commodities that have high value everywhere and to everyone. Even Beattie's description of his own experience with the scientist-astronaut selection process reflects such an outlook. A Navy jet pilot, the lone geologist applicant, and already intimately involved with Apollo, he assumed that he would be a logical and obvious choice for the job and that the selection process was "only a formality" (p. 177). He was stunned to find out he was wrong.

Science does not need to shill for the next Tang or cordless drill, but it does need to integrate the rest of the world in its unique outlook. Its value is never patently obvious or all-appealing. Both scientists and their interpreters—and here I include historians—must learn to listen, to participate, and to persuade.

MAURA PHILLIPS MACKOWSKI

Philip Atkins. *Dropping the Fire: The Decline and Fall of the Steam Locomotive.* vi + 106 pp., illus., figs., tables, apps. Bedfordshire, U.K.: Irwell Press, 1999. £14.95.

This book chronicles in encyclopedic fashion the almost universal dethroning of "King Steam" in the second half of the twentieth century. It also

seeks to explain "the indecent haste" of steam locomotives' replacement by diesel and electric engines, or, as one observer put it in the mid 1960s, "the worldwide contagion to be rid of steam at a rate beyond all common-sense" (p. v). In addition to railroad enthusiasts, *Dropping the Fire* will be of interest to specialists in the history of transport and technology, for it offers perhaps the only comprehensive account of one of the most important developments in land transportation over the last century.

After a brief global survey of locomotive production and operation, Philip Atkins devotes four chapters to the United States and one each to the leading European builders as well as to other countries and regions. He concludes with two chapters on the demise of the steam-locomotive industry and last-gasp efforts to improve steam-engine technology. A notable theme that emerges from his discussion of the various national experiences with steam-locomotive construction is the global reach of certain design traditions, even to the point of transcending international conflict. The export of American steam engines to the Soviet Union, for example, "injected U.S. technology into Russian practice" (p. 57) and from there into mainland Chinese practice, so that at the height of the Cold War the Soviets and the Chinese were both manufacturing locomotives that were "purely American in concept" (pp. 58, 84).

The author notes that, between 1960 and the mid 1990s, the steam locomotive became extinct in all the major railroad countries save China, which expected to eliminate main-line steam traction at the start of the twenty-first century. By the early 1960s, of the twenty-some nations that had ever engaged in serial production of steam engines, only two—China and India—were still actively building them; and 1998 was the first year "since the reign of King George III [1760–1820] in which no steam locomotives whatever [were] built anywhere in the world" (p. 102).

Atkins maintains that the steam engine's "fall from grace" stemmed not so much from "prejudice" as from genuine concern over the long-term procurement of coal, combined with the rapid emergence of a superior alternative, the diesel-electric. The oil crises and price hikes of the 1970s delayed the final demise of steam in several countries. Meanwhile, the development of new technologies that promised to overcome the perennial problems of low thermal efficiency and smoke emission might have returned steam traction to a more permanent viability, as the author suggests in wistfully describing what, to his

mind, would constitute “the ultimate steam locomotive” (p. 100), one that would incorporate the latest in proposed technical improvements. But even though most countries, including the United States, initially did not envision the rapid or even complete displacement of steam by diesel-electric engines, “once steam was pushed into second place, its continued operation became disproportionately uneconomic, thereby accelerating its elimination” (p. 102).

The abundance of factual detail this book offers makes it most handy as a reference work on the last century or so of steam-locomotive technology and design, although the lack of an index detracts somewhat from its usefulness in that regard. Atkins includes a large number of striking photographs and diagrams of locomotives, often with lengthy captions. He might have integrated the illustrations more fully and clearly with the text, however, referring to them by figure numbers in the text and streamlining the captions. In addition to the illustrations, the author provides a useful list of references at the end of each chapter and several dozen tables and appendixes presenting data on locomotive construction and operation in various countries. The story is a fascinating one, and Atkins certainly supplies the images and details to capture the steam locomotive in its final glory and decline worldwide.

STEVEN J. ERICSON

Naomi Oreskes (Editor). *Plate Tectonics: An Insider's History of the Modern Theory of the Earth*. With **Homer Le Grand**. xxiv + 496 pp., illus., notes, index. Boulder, Colo.: Westview Press, 2002. \$32 (cloth).

In this book seventeen scientists, all of whom were graduate students or youthful researchers at the time, describe their participation in the investigations that led to the formulation of the theory of plate tectonics in the late 1960s. They provide vivid accounts of the senior scientists with whom they worked and of the unfolding of events. Following a historical overview by Naomi Oreskes, the chapters are grouped under four headings: “From Paleomagnetism to Sea Floor Spreading,” “Heat Flow and Seismology,” “The Plate Model,” and “From the Oceans to the Continents.”

Much new light is thrown on key discoveries as we read descriptions by different scientists of how and when they learned of them and how they responded. These accounts clearly show that the investigations were not made in response to any sense of a Kuhnian crisis in the earth sciences. They were made in a euphoric postwar

period when funding was generous and newly available instrumental techniques were being applied for gathering information, particularly about the ocean basins. None of the observations were theory laden, nor were experiments conducted for hypothesis testing. On the contrary, at least two authors ask why it took so long for scientists measuring paleomagnetism and heat-flow values to formulate the sea-floor-spreading hypothesis from data they had been collecting through the 1950s. One geophysicist suggests that it was because most of the scientists involved were physicists, like himself, who had been trained to view hypothesis testing as the only proper approach to science. But they all had so much noise and scatter in their data that they could not extract the quantitative results necessary to frame hypotheses. He remarks that it took a geologist like Harry Hess, with his skills in data synthesis and pattern recognition, to place the accumulating results in a broad perspective.

This brings us to another theme in the book: the frequency with which two or more scientists formulate similar ideas separately but simultaneously. To take one example: Hess preprinted his big picture in 1960 and published it in 1962; Robert S. Dietz published his in 1961. Dietz proposed that the ocean floors consist of basaltic lavas that erupt at the ridges, split apart, and move to either side until they descend back into the mantle at the trenches. He named this process “sea-floor spreading.” Hess argued that the ocean floors move in a similar fashion (to which he gave no name) but consist of a thin rind of serpentinite on the peridotite mantle. Dietz’s basalt and his name for the process are universally accepted today; Hess’s serpentinite is tactfully ignored. Nevertheless, many of these authors credit Hess with the idea of sea-floor spreading and mention Dietz only in passing. Here lies a conundrum for historians of science.

Both Hess and Dietz (and Arthur Holmes thirty years earlier) proposed mantle convection as the driving force that moves the sea floors from the ridges to the trenches. But two authors point out that the ridges, with their faulted topography, heat-flow patterns, and seismicity, are better explained if they are seen as passive features formed by the partial melting and upwelling of hot mantle wherever global stresses open a fracture. This would explain how some ridges maintain their positions midway between continents and resolve the mystery of how a ridge can be split into short segments abutted at both ends against the cooled walls of an adjacent ridge flank—a most unlikely configuration for a thermal convection cell.

Several authors address the special problems of continental tectonics, which often show clear evidence of interior stretching or other modes of deformation that appear to be inconsistent with the conception of plates as rigid features. To date, however, no evidence of the nonrigidity of the major plates has been detected; and so, with a few adjustments here and there, the model remains in place.

The one note of dissent was raised by the geophysicist Gordon J. F. MacDonald (I note, with regret, that MacDonald died suddenly in May 2002), who argued in the early 1960s that continental structures extend to depths of up to 300 miles, a problem for models that describe thin plates moving on a flowing mantle. His conclusion, based on geophysical constraints imposed by data on heat flow, gravity, seismicity (including free oscillations), and information provided by the behavior of earth-orbiting satellites, was effective in (briefly) delaying the widespread acceptance of sea-floor spreading and plate tectonics. MacDonald's chapter in *Plate Tectonics* points out that the problems he raised back then still have not been resolved; he added that recent, greatly improved seismic observations were showing that subplate structures extend to depths of at least 150 miles. MacDonald described the swift and well nigh complete acceptance of plate tectonics by the earth science community circa 1968 as a manifestation of the herd instinct. Naysayers with substantial evidence to offer are always good for science.

Earth scientists from students through professors emeriti will be royally entertained and informed by this book, as will general readers who are interested in science. Historians of science should not be without it. It is an extremely valuable addition to the history of one of the less exact but very important branches of science.

URSULA B. MARVIN

Paul Andrew Mayewski; Frank White. *The Ice Chronicles: The Quest to Understand Global Climate Change*. Foreword by **Lynn Margulis**. xxv + 233 pp., illus., tables, figs., refs., index. Hanover, N.H.: University Press of New England, 2002. \$24.94 (cloth).

In 1993, after five years of hard drilling, the Greenland Ice Sheet Project Two (GISP2) team struck bedrock at a depth of 3.2 kilometers and brought to the surface the last section of the Northern Hemisphere's longest and oldest ice core. It was a "time machine" containing ice, air bubbles, dust, and chemicals deposited up to 110,000 years ago. Paul Andrew Mayewski, a

prominent ice chemist and chief scientist of the project, coordinated the efforts of some twenty-five American universities. One of his colleagues, the author of a similar book on ice cores and climate, has referred to Mayewski as the "'hero' [who] made it happen by solving each of the myriad problems that arise over the long years of planning and doing" (Richard B. Alley, *The Two-Mile Time Machine* [Princeton, 2000], p. 23).

Frank White, an accomplished science writer, has teamed with Mayewski to recount two kinds of "ice chronicles," weaving Mayewski's adventures on the world's glaciers and his personal scientific insights into a longer and more transcendent narrative of how ice cores and ice chemistry chronicle the climate past and illuminate the present and future. The adventures include being lost in Antarctica and getting caught in a snow storm with 120 mile-per-hour wind gusts; personal insights include descriptions of how the ice core community "got organized" and how the GISP2 drilling site was chosen. Numerous short technical sidebars on climate science pepper the text. These discuss chemical measurements in ice cores (two pages), the ocean's conveyor belt (two pages), and how our understanding of the ozone hole evolved (one page). The larger grand narrative is reserved for chapters on "the rise and fall of civilizations" (with deference to Hubert Lamb) and how the current debate about global climate change and policy options look from the long-term perspective of ice cores.

At the heart of the book, and arguably its most exciting chapter, is an account of the discovery, using GISP2 data, of Rapid Climate Change Events (RCCs)—sudden, massive reorganizations of the climate system. For example, about 12,800 years ago, temperatures dropped 30 degrees Celsius in central Greenland in less than a decade. There is evidence that other areas of the world became cooler and drier as well. About 700 years later, climate conditions rebounded just as abruptly. This event, called the Younger Dryas, is clearly marked by dust and chemicals in the ice. It turns out that natural climate changes have occurred suddenly and somewhat periodically many times in the past—"even before human beings began to alter the Earth's climate on a vast scale" (p. 82).

The book has a strong environmental focus that reveals Mayewski's love for the purity of the glacial region. The authors are very generous, with valuable citations to the work of a large number of colleagues. Yet the conclusion of the book is rather bland, arguing for more "good science" and "compromise" in "our complicated

world of politics, technology, and economics" (p. 200). Overall, *The Ice Chronicles* provides a starting point and reference source for historians interested in ice core drilling and climate issues in general. It could profitably be read in tandem with Alley's book, mentioned above.

JAMES RODGER FLEMING

Jean-Pierre Dupuy. *The Mechanization of the Mind: On the Origins of Cognitive Science.* Translated by **M. B. DeBevoise.** (New French Thought.) xiv + 210 pp., bibl., index. 1994. Princeton, N.J./Oxford: Princeton University Press, 2000.

Between 1946 and 1953, a series of ten conferences sponsored by the Josiah Macy, Jr., Foundation took place in New York City. These meetings, known thereafter as the Macy Conferences, were organized in order to combat the threat of "growing specialization, professionalism, and isolation of scientific disciplines" and were aimed at "knock[ing] down the artificial barriers that stand in the way of specialists speaking to each other, in order to hasten the eventual unification of science" (p. 81). With a utopian eye toward world peace, the Macy Conferences brought together many distinguished scientists from different disciplines, including the neurophysiologists Warren McCulloch, Walter Pitts, and Ralph Gerard, the physicist Norbert Wiener, the psychologists Wolfgang Koehler and Karl Lashley, the psychiatrist Lawrence Kubie, the anthropologist Margaret Mead, the polymaths John von Neumann and Herbert Simon, and many others. Within this remarkable group there was a core of six individuals who constituted the "cyberneticians"—McCulloch, Pitts, Wiener, von Neumann, Arturo Rosenbleuth, and Julian Bigelow. Despite their different disciplinary backgrounds and metaphysical assumptions, these men were committed to two fundamental principles: that thought is a form of computation that is not restricted to human brains but instead belongs to a particular class of computing machines; and that we will ultimately be able to understand the fundamental elements of mental life—for example, semantics and intentionality—on the basis of physical law. In *The Mechanization of the Mind*, the French philosopher Jean-Pierre Dupuy, one of the heirs to the cybernetic legacy (he is a protégé of Heinz von Foerster, secretary to the last five Macy Conferences and founder of the "second order cybernetics" movement), has produced an ambitious and penetrating sketch of this first family of cybernetics.

Dupuy does not provide a detailed genealogy of the cybernetics family, material that is more extensively reviewed elsewhere (see, e.g., Steve J. Heims, *The Cybernetics Group* [MIT, 1991]; and Heims, *John von Neumann and Norbert Wiener: From Mathematics to the Technologies of Life and Death* [MIT, 1980]), but, rather, presents a multilayered intellectual history of the early cybernetics movement. Drawing on both analytic and postmodern philosophical traditions—Dupuy divides his time between Stanford University's Center for the Study of Language and Information and the Applied Epistemology Research Center at the École Polytechnique in Paris—he deftly covers a wide variety of technical material, beginning with the mathematical developments of Kurt Gödel, David Hilbert, and Alan Turing that provided the theoretical foundation for cybernetics work. In particular, Dupuy draws attention to the importance of Turing's theorem that every mechanical procedure can be modeled by an abstract machine (thereafter known as the "Turing machine"), and he points out that Turing's theorem was both misunderstood and axiomatized by a number of cyberneticians eager to advance their research program. Dupuy describes the seminal contributions of McCulloch, who (in cooperation with Pitts) showed how a finite Turing machine might be embodied in a relatively simple network of neurons. In contrast to previous accounts that highlight the contributions of Wiener to the Macy Conferences, Dupuy champions the role of McCulloch, emphasizing his critical work in the genesis of the conferences and in the stewardship of the cybernetics group. This intellectual history is interwoven with philosophical reflection: Dupuy considers the epistemic/ontological status of models in McCulloch's work, the connection between post-Heideggerian metaphysics and the cyberneticians' dream of thinking machines, and the failure of cyberneticians to embrace the contemporary and complementary developments in phenomenology.

Two of the aims of *The Mechanization of the Mind* are to demonstrate the linkage between early cybernetics work and the theoretical foundations of cognitive science, artificial intelligence, and complexity theory; and to reveal why many of these fields are ashamed of their "cybernetic heritage." Despite the brevity of his book, Dupuy succeeds in identifying a number of important linkages between cybernetics and the robust tree of cognitive science, with its intersecting branches of AI, cognitivism, and philosophy of mind. The break between cognitive science and its "poorly loved parent" is left un-

explained, however. Dupuy instead draws attention to a number of ironic twists in the history of cybernetics and to missed opportunities between the cyberneticians and those behavioral scientists, social scientists, biologists, and philosophers that participated in the Macy Conferences. Indeed, he argues that despite the fact that the “new discipline [of cybernetics] saw itself as the avant-garde of science . . . the truth remains that cybernetics managed to botch what might have been productive encounters with any number of potential allies” (p. 144). The book poignantly illustrates the double edge of interdisciplinary work, revealing how the cyberneticians chiseled out a revolutionary model of the mind while simultaneously severing themselves from their rightful legacy.

DAVID MILLETT

M. L. Tina Stevens. *Bioethics in America: Origins and Cultural Politics*. xvi + 204 pp., index. Baltimore/London: Johns Hopkins University Press, 2000. \$39.95.

Bioethics is one of the most interesting intellectual disciplines to appear in the twentieth century. It first appeared in the mid 1960s as a result of the efforts of a tiny handful of scholars, primarily with backgrounds in medicine and theology, who worked outside their traditional disciplinary homes at an array of think tanks and institutes. By the late 1990s bioethics had grown into a flourishing discipline with specialized departments, graduate degrees, encyclopedias, dictionaries, conferences, commissions, awards at graduation ceremonies, and all the other accoutrements indicative of intellectual vigor. M. L. Tina Stevens has correctly identified bioethics as a field meriting careful analysis as an intellectual phenomenon in its own right, as a result of the power it wields within biomedicine and the influence it has in the realms of public policy and public life. Unfortunately, the analysis she provides of the rise of bioethics is too narrow in its focus and too far off the mark in its overly simplistic analytical framework to do justice to the richness of the subject.

Stevens correctly sees the origins of bioethics in the United States. Bioethics has been and even today remains a field with deep roots in American values and culture. Why this is so is an object of much discussion among old hands in the field as well as non-Americans who would like to make bioethics a bit less American in focus and style. Stevens thinks that the way to get a handle on the rise of this peculiarly American intellectual enterprise is to focus on an institution

that was critical in the creation of the field—the Hastings Center.

The Hastings Center is a freestanding think tank founded in 1969 by Daniel Callahan, a philosopher, and Willard Gaylin, a psychiatrist. The center, which was originally located in Hastings-on-Hudson, New York, but has since moved up the Hudson River to a location near West Point, has indeed had an enormous influence on the field of bioethics in America, especially during the 1970s and early 1980s. This despite the fact that it has never had more than ten full-time senior associates in residence at any given time and, for the majority of its existence, has had almost no endowment and a fragile bottom line.

Stevens puts all her analytical chips on the Hastings Center as the key institution in American bioethics. She is especially interested in what happened there during the early 1970s. Her main claim is that the center, and thus the field, served as a moral handmaiden to biotechnology and medicine. Bioethics bought its success by working hand-in-glove with biomedicine.

The appearance of the Hastings Center can, she maintains, be seen as synonymous with the appearance of bioethics. The appearance of bioethics can be explained as a marriage of convenience between the center and a biomedical community worried about the legacy of “post-atomic ambivalence” toward technology. Intellectuals such as Gordon Rattray Taylor, Donald Fleming, Jacques Ellul, Theodore Roszak, and Ivan Illich, among others, were making Americans nervous about where biotechnology and medicine might take them. Bioethics and the Hastings Center arrived just in the nick of time to allay American society’s anxieties and to work closely with physicians and biologists to let them pursue their agenda. That agenda, according to Stevens, included the redefinition of death to the far less credible standard of brain death so that the field of organ transplantation could harvest the requisite parts and the promotion of a dubious “right to die” where the right to stop treatment preempted a parallel movement to treat the dying with dignity and compassion.

I was at the Hastings Center during some of the time frame that occupies Stevens’s attention—the last half of the 1970s—as a research assistant and then postdoc. I had no responsibilities in the events that Stevens presents, but I did have a nice seat in the peanut gallery to watch what was going on. I don’t think Stevens gets the story of either Hastings or the rise of bioethics right. And the use of brain death and the right to die as the key examples of how bio-

ethics gave cover to questionable medical practices is not plausible at all.

While the Hastings Center was important—in fact, critical—to the development of the field, it did not serve the Vatican-like role that Stevens assigns to it. Hastings always had the Kennedy Institute at Georgetown, an institution with explicit religious ties that the center lacked, as a rival. It also was so far out on the intellectual fringe in the early 1970s that very few mainstream scientists and doctors were drawn to it. Its founders were proud mavericks who, for various reasons, felt more comfortable outside the mainstream of medicine, science, and university life than inside it.

Stevens captures none of this. Seemingly adrift in the paperwork she had access to in the center's files, she offers no insights about the intellectual climate in bioethics at the time, the battles between theologians, philosophers, social scientists, and doctors to set the disciplinary tone for the field, or the oftentimes hilarious efforts to keep the center afloat from one month to the next. If the center had cooptation issues they were with foundations—not with the mandarins of the National Institutes of Health, MIT, or Ivy League medical schools.

Worse, her claim that the Hastings Center was somehow used to stifle a robust technological skepticism about biomedicine is both inadequately supported and exceedingly implausible. The figures she cites as at the vanguard of the antitechnology movement had no real following and no prospect of finding one. Mainstream science and medicine, as represented by expanding budgets at the NIH and big science projects such as the creation of an artificial heart, were not in any danger of being shut down by antitechnology critics. While worries about recombinant DNA and fears about the misuse of large-scale genetic testing were soon to come, they had nothing to do with the kinds of epistemological critiques in evidence in the writings of a Roszak or an Ellul.

Perhaps the strongest case against Stevens's claim that Hastings was a willing dupe of big science is that almost no one on the staff during the 1970s was perceived as in any way friendly to biomedicine by most of those in biomedicine. Callahan, Gaylin, Robert Veatch, Ruth Macklin, and Marc Lappe were hardly individuals likely to get invited to Woods Hole, a Gordon conference, a National Academy of Sciences panel, or an Association of American Physicians meeting. While not antiscience or antimedicine, all were more than willing to be highly critical of modern medicine and its technological promises, a po-

sition that can be found today especially in Daniel Callahan's recent writings.

What really is wrong with Stevens's case is that she uses the legal recognition of brain death and the creation of the right to withdraw treatment from the terminally ill in American law as evidence of a conspiracy between bioethics and the biomedical mainstream. The driving force behind brain death was as much the need to know when to turn off life-supporting technology as it was the need to find bioethicists to pave the way for surgeons to obtain organs from cadavers. State legislators were hardly bamboozled by moral flimflam artists into enacting brain death laws. Some states did, and some did not. Some, such as New Jersey, kept tweaking brain death well into the 1990s. Bioethicists were neither organized enough nor committed enough to brain death to be capable of pushing through legislation that hospitals, lawyers, religious groups, and the public did not want to see enacted.

The same holds true with the right to die, only more so. Certainly the United States has never really come to terms with the lack of humane care it afford its elderly, severely disabled, and dying citizens. But Americans were not victimized by ethicists fronting for doctors looking to reduce costs in deciding to enact legislation allowing the withdrawal of treatment and the use of advance directives to guide care for the incompetent. Americans were so nervous about losing control of their dying, winding up suspended in a kind of limbo that they could not tolerate, and so committed to the value of the right to control their own destiny even in how they died, that the creation of a right to die needed no help from bioethicists. True, bioethicists did help; but the law was already on the move. A decade of earlier cases based on religious freedom concerning the rights of Christian Scientists and Jehovah's Witnesses to refuse medical treatment make it very clear that when it comes to the right to die bioethics was closer to jumping on a bandwagon than to pulling it to get it started.

Bioethics in America contains an insight or two, but it fails in its main task: to explain the rise of bioethics. The need to do so—and to do so with a critical eye—still remains, especially given the prominence bioethical issues now have on the national and international scene. Coopted or regressive bioethics may well be and may well have been in its earliest days, but the case for such claims is not yet proven.

ARTHUR L. CAPLAN

Michel Dubois. *La nouvelle sociologie des sciences.* (Sociologies.) viii + 256 pp., figs., tables.

Paris: Presses Universitaires de France, 2001. Fr 198 (paper).

This book is a collection of presentations and papers that includes several chapters based on the author's 1997 doctoral dissertation. Its purpose is to introduce current trends in the sociology of science, both in France and in the Anglo-Saxon world, in a manner that documents the liveliness of this academic community. Michel Dubois offers a spectrum of studies ranging from a comprehensive inventory of European research centers in sociology of science to explorations of controversies such as the Sokal affair. The introduction sets out the two goals of the book: to present a theoretical and methodological overview of the nature of the new sociology of science and to pursue a sociological inquiry into the way this scientific domain has spread over a wide range of academic fields.

The critical investigation of the various "programs" in science and technology studies focuses on the successes and failures of post-Mertonian sociology of science vis-à-vis its ambitions. Dubois favors studies using the frameworks of "constructivism," "relativism," and "relationism" that open the scientific process to a wide range of explanatory factors rather than reducing it to a matter of external social influence or internal coherence. The thesis of the book is well expressed in the following sentence: "Taking into account this contextual dimension [notably cognitive interests and disciplinary constraints] shouldn't lead us to discard the influence of institutional structures or cognitive norms on the behavior of researchers, for an emphasis on extrascientific 'interests'; but it should lead to showing their situatedness which stems from the analysis of the way researchers find 'good reasons' to act in compliance with immediate or less immediate imperatives with which they are confronted" (p. 233). In fact, the whole book pleads that we not lose sight of the syncretism of causes that explain scientific developments by means of commentary on the limitations of three particular approaches (or scientific networks): the Parex group (T. Shinn, M. Mulkay, G. Lemaine, P. Weingart, etc.), which tries to show the multidimensional activity of science; the "strong program" proponents (T. Pinch, D. Bloor, B. Barnes, H. Collins, etc.), who defend a vision of causality in science associated with the cognitive interests of scientists; and the "socioconstructivism" group (B. Latour, M. Callon, K. Knorr-Cetina, S. Woolgar, M. Lynch), which stresses the scientific practices observed in laboratories.

These groups are the focal point of an analysis that places them in the wider evolution of the sociology of science, both in the United States, with the development of science and technology studies departments and the influence of Kuhn, and in France, where Dubois points to forerunners like Duhem and Le Roy. The discussion of these groups is used to explain the failure of the "strong program" (one chapter), the misunderstanding of relativism in the sociology of science (one chapter), and the validity of Alan Sokal's attacks against the field (two chapters). The purpose is to put forward an insider's view of the sociology of science from Merton to the present, emphasizing the continuity of its evolution (where more radical perceptions would assume that there have been several revolutions), and to emphasize how the controversies within the field in fact promote a more complex vision of it. In that regard, this is a successful attempt that helps explain the respective positions of the different schools and research networks. One will, however, hardly be convinced by Dubois's principal argument unless one already has insights into the various fields of study. This latter knowledge should help one read Dubois's work properly (against his intention?) by preventing one from making general arguments without delving into the specifics of the sciences in question.

FLORIAN CHARVOLIN

Jorge Niosi; André Manseau; Benoît Godin. *Canada's National System of Innovation*. xvi + 222 pp., illus., figs., tables, bibl., index. Montreal/London: McGill-Queen's University Press, 2000. \$65.

It has been said that innovation is a fuzzy concept, difficult to define even for historians and theoreticians of economic and industrial policy. Imagine the challenges facing policy makers. After all, it is they who are at the front lines of shaping directions for any given country's R&D agenda. Now consider a more ephemeral notion: a national system of innovation (NSI). Students of the NSI, such as Jorge Niosi—a respected scholar of the management of technology—will define it as "a system of interacting private and public firms, universities and government laboratories aiming at the production and use of science and technology within national borders" (p. 6). This is vague enough to provide a useful schema by which one can undertake specific case studies that ultimately underscore the specificity of any given research "model." In the early 1970s, for example (without benefit of the NSI methodology), the political scientist Robert Gil-

pin referred to Canada's S&T policy as *sui generis*. By this he meant that Canada's approach to designing and investing in research policies was unique. It still is.

Canada's National System of Innovation is an attempt to capture in an analytical framework the evolution of Canada's science and technology development. It is a unique road map of Canadian data and achievements in research and development, presented through the filter of institutional, economic, and social commentary. We've seen elements of this before. In the mid 1970s the Organization for Economic Cooperation and Development (OECD) was engaged in a similar analysis when it produced its three-volume set on the research systems of several OECD countries, including Canada. In this earlier period the OECD was preoccupied with changes to institutions and the governance of S&T and closely examined the differences in such approaches from country to country, with a view to gleaning patterns and trends.

This work is more analytical, benefiting as it does from various statistical series showing the changes to Canada's R&D investments by sector: industry, government, and university. It is complemented by results of surveys that Niosi and his coauthors have conducted on the rise of cooperative R&D and Canadian R&D abroad. It gives the reader a sweeping view of how Canadian R&D performance has evolved from its infancy to the present; how universities have become the major beneficiaries of government largesse since World War II; how Canadian industrial R&D—the presumed motor of a national innovation system—remains backward compared to that of its G-7 siblings; and how government R&D has undergone significant transformation in terms of institutional experimentation, with little in the way of concrete results to show for it.

The book is best in describing the details of Canadian performers in R&D—domestic and international. The authors are conscious of the importance of regional and local activity and blend their analysis with good comparative data on how Canadian R&D has fared in the international arena; they present surveys and investment data. They point out the difficulties of governance caused by Canada's geography, ongoing federal and provincial squabbles, significant foreign ownership of the industrial base, and the lack of defense R&D investment. The chapters on patent trends and venture capital show clearly how unique the Canadian R&D scene has become as a result of its proximity to the United States. The more speculative foray on the poten-

tial for a North American Innovation System comes to the obvious conclusion: the differences in the three national systems of innovation outweigh the similarities; North America is more a "space" than a system when it comes to a common knowledge agenda.

What can we learn about Canada from this book? Quite a bit, as it turns out. It is remarkable to see the transformation of Canada's national innovation system over the past half-decade. But the reader should fully understand the outcomes: national policies designed to shape the direction of S&T—and their results—can be just as accidental as they are planned. Referring to such directions as "systems" grossly overstates the notion that states can design and develop large-scale strategies for innovation. Luck, timing, market and information failures, geography, and key personalities have just as much to do with the development of the NSI as do regimes of conscious planning.

PAUL DUFOUR

John P. Jackson. *Social Scientists for Social Justice: Making the Case against Segregation.* xii + 291 pp., notes, bibl., index. New York: New York University Press, 2001. \$45 (cloth).

John P. Jackson's study investigates the history of social scientists, especially social psychologists, whose research on prejudice and discrimination was used by the NAACP's Legal Defense and Education Fund (LDEF) in various briefs leading up to the U.S. Supreme Court's *Brown* decision of 1954. Jackson challenges what he considers the "common view" today "that social scientists stepped outside their role as scientific experts when they testified that segregation was damaging" (p. 5). He offers a critique of revisionists of both the left and the right who have argued that the NAACP's case in *Brown* was based on dubious interpretations of social science studies conducted by activist social scientists.

Jackson traces the history of research on racial attitudes and behavior in the 1930s and early 1940s and argues that most of it was carried out in carefully constructed academic experiments independent of any connection with policy-advocacy groups. By the late 1930s, he finds, social psychologists had concluded that prejudiced attitudes were formed not by contact with minorities but merely by contact with prevalent stereotypes of minorities. The distinction between attitude and behavior enabled social psychologists to discredit William Graham Sumner's suggestion that "stateways cannot change

folkways” with studies in the early 1940s that showed that persons who said they opposed desegregation of facilities in various settings in the North actually accepted the change once it was instituted by authorities. Jackson attributes to E. Franklin Frazier and other sociologists the idea that segregation had created “pathological” aspects of African American culture and notes that Kenneth Clark combined this construct with Kurt Lewin’s concept of ethnic self-hatred to make the psychological “damage” caused by segregation a central subject for investigation by social psychologists in the late 1940s and early 1950s. While he concedes that there were some contradictions in Clark’s testimony about his doll tests, the author insists that this was a very small part of the NAACP’s presentation of social science evidence and notes that the Supreme Court did not refer to those tests in Chief Justice Earl Warren’s opinion in *Brown*.

In chapters based on archival research on the American Jewish Congress’s Commission on Community Interrelations and the NAACP’s LDEF, Jackson emphasizes the efforts of social scientists to maintain their “objectivity” in research memoranda and testimony completed under the auspices of these agencies. He notes that social scientists working for the commission sought to maintain a distinction between themselves as “objective” scientists doing research and the organization’s “action people”—lawyers, social workers, and so forth—who were concerned with policy issues. In a detailed examination of the LDEF’s use of social scientists as expert witnesses in desegregation cases, Jackson also observes that scholars frequently pulled back from doing things that might compromise their objectivity. He outlines various strategies by which social scientists limited claims, issued caveats, and distinguished between “facts” and “interpretation” in efforts to convince the public of the objectivity of their research on race prejudice and discrimination. The author is particularly keen to defend the social scientists against the charge that they were “naïve” in believing that the elimination of legal segregation would lead automatically to dramatic changes in racial attitudes. Jackson emphasizes that Gordon Allport, Clark, and other social scientists saw the abolition of segregation as a necessary first step to better race relations but not a sufficient strategy for ending racism.

In focusing on social psychology, Jackson tends to slight other social science disciplines, giving scant attention, for example, to the sociologist Robert Merton’s pathbreaking research on racial attitudes and behavior. He demon-

strates convincingly that social psychologists strove earnestly to maintain their “objectivity” in their work with the NAACP and describes a variety of strategies by which they tried to do so. The book lacks, however, an effective conceptualization of what “advocacy” and “objectivity” meant in the social sciences from the 1930s to the 1950s. Therefore it is difficult to judge how effective the social psychologists actually were in achieving this goal of objectivity. The book offers no comparison between the objectivity of research in this area and that of research on other topics in the period, nor is there much discussion of the legacy of the testimony of social scientists in the *Brown* litigation for subsequent cases involving testimony by social scientists. The author is reluctant to place the social scientists in a broader ideological context.

On the whole, though, Jackson has written a fine monograph that traces the research of social psychologists on racial attitudes and discriminatory behavior from the 1930s to the 1950s, providing a sensible corrective to some of the revisionism on this topic. As we approach the fiftieth anniversary of *Brown*, it is clear that social scientists played a significant role in undermining the legal rationale for segregation in the 1896 *Plessy* decision and in making the argument that “separate cannot be equal.”

WALTER A. JACKSON

Baruch S. Blumberg. *Hepatitis B: The Hunt for a Killer Virus*. 272 pp., table, illus., app., bibl., index. Princeton, N.J.: Princeton University Press, 2002. \$27.95 (cloth).

Baruch Blumberg received the Nobel Prize as a result of his discovery of a marker for the hepatitis B virus and his efforts in creating a proto-vaccine against the disease that it causes. This book, despite its title, is not a study of the search for the hepatitis B virus but, rather, a memoir—a retrospective, at seventy-five, of “my life in science.” It is written in a beautiful and engaging style that most professional writers would envy.

From the standpoint of the history of science, this book by an eminent scientific practitioner is noteworthy because the intellectual framework that holds it together is a fairly sophisticated one that resonates with modern academic thinking. Blumberg says that “accounts of scientific research are often presented in a . . . form suggesting that the process was planned in advance to follow a logical and ordered sequence.” This is not the case, he says, because “science proceeds from one uncertainty to another,” often

moving in unexpected directions far from what was originally assumed (p. 3).

If Blumberg had been able to do what he set out to do—move away from this myth of science and replace it with a more accurate account of how research is actually practiced—this book would be invaluable. Unfortunately, that is not the case. Blumberg is not able to do what he set out to do because at the heart of his own career is an instance of serendipity in science that is so extreme and so far outside the norm that it is not possible for him to process it adequately or deal with its implications.

Blumberg's claim to fame is that he discovered the "Australian Antigen," which turned out to be the surface protein of the hepatitis B virus. That discovery helped revolutionize the study of and fight against hepatitis B. However, he chanced upon this discovery as the result of research in a radically different field: "the systematic search for inherited biochemical and immunological variation ('polymorphisms') in human populations . . . [with] the faith—that in due course we would identify inherited differences in susceptibility and resistance to disease" (pp. 5–6). The scientific love of his life involved genetics and disease. He originally thought he had discovered a marker for a gene that was associated with susceptibility to leukemia, leprosy, and hepatitis.

While the book makes it appear that through slow, careful, and conscientious hypothesis creation Blumberg came to the conclusion that he had discovered a viral rather than a genetic antigen—a classic example of the interaction of chance and "the prepared mind"—the fact is that he was strongly resistant to the idea that it could be a virus rather than an inherited trait when it was proposed to him (see William Muraskin, *The War against Hepatitis B: A History of the International Task Force on Hepatitis B Immunization* [Pennsylvania, 1995], pp. 47–48). His reluctance was very human and understandable, since virology was not the field he was interested in or knowledgeable about.

While this early work led to his patenting the idea for a proto-hepatitis B vaccine and, ultimately, to the Nobel Prize, its peculiar nature made it an infirm basis on which to build a secure scientific reputation. Blumberg spent the next thirty years of his life in hepatitis B work. The question that the reader has to ask is whether this subsequent work made a major contribution to his adopted field. The book makes it very hard to tell. He seems to claim on numerous occasions that his work was pioneering and groundbreak-

ing, but then so severely hedges those claims that they seem to vanish.

By the end of the book the reader is left with the feeling that this story is more a cautionary tale than an inspirational work. Serendipity led Blumberg to early fame that pushed him away from the specialty that he loved and into a field where he was neither totally accepted nor fully at home. Blumberg's life is worth study by historians of science for lessons to be learned, but this book won't help them.

WILLIAM MURASKIN

Jordan Goodman; Vivien Walsh. *The Story of Taxol: Nature and Politics in the Pursuit of an Anti-Cancer Drug*. xiii + 292 pp., illus., bibl., index. Cambridge: Cambridge University Press, 2001. \$27.95 (cloth).

In the search for medical therapies, researchers are periodically seduced by the notion that nature has better solutions than anything synthesized in a lab. We see this in the pursuit of sea coral, human tissue, and plant extracts for a variety of medical uses. "Better," of course, can mean more effective, better tolerated, cheaper to develop, easier to procure, or publicly favored over risky alternatives. Jordan Goodman and Vivien Walsh have written a well-researched, careful account of the vagaries of such efforts and demonstrate that the value of natural materials is contingent on multiple social, political, and technical conditions over time. At the same time, they provide an important exemplification of twentieth-century public-private research relationships, the privatization of natural resources, and the backstage political activities involved in determining what is in the "best interest" of the public.

The book uses actor-network theory to chart the complicated history of *Taxus brevifolia* and its product taxol. In contrast to accounts that begin with the "discovery" of a molecular formulation of taxol in the 1980s, the authors emphasize that a durable network of participants had to be maintained in order to prove that taxol was effective in treating cancer in a commercially viable way. They connect participants and events within several domains, including cancer research circles, federal policy worlds (environmental, medical, and scientific), bark collection labor economies, and environmental advocacy groups. Just as important, however, are details about contracts, laboratory techniques, and the adoption of particular assays—key to stabilizing networks of diverse actors. For example, the appeal of taxol to biologists as a tool for studying cell dynamics and its use as a test compound for

xenograft models sustained an interest in continued development even in the face of problems with formulation, yield, and political concerns.

The authors use a broad range of sources, relying heavily on institutional records (including documents and memos from the National Cancer Institute, the U.S. Department of Agriculture, and corporate archives and *Congressional Record* texts), as well as literature about forestry practices and chemotherapy theories. Interviews and popular media coverage enliven the accounts of the debates that pitted environmental concerns against cancer advocacy. In fact, there is some imbalance between the richness of these accounts and the encyclopedia-like descriptions of the laboratory and policy arenas. In a subtle way, this perpetuates an image of nature, rather than the lab, as the site of unpredictable, colorful human dynamics.

The book is divided into three sections: an introduction that contextualizes cancer chemotherapy and screening programs, a section that chronicles research activities from 1962 to 1989, and a final part that turns to conflicts between interests as taxol came to market through a different form and venue than what was first imagined. It is difficult to know how best to organize such a large volume of data. If the point was to illustrate actor-network theory, however, it might have been more effective to build the story around the recruitment and upkeep of network participants, rather than resorting to chronology. As it is, the theory is mentioned in the first and last chapters but not effectively put to use throughout. For example, a central undercurrent is the simultaneous redefinition of both "nature conservation" and "cancer therapy" during a time when the roles of state and private commercial interests were shifting. An opportunity to illustrate the extraordinary work involved in negotiating roles and relationships in such juxtapositions was missed by emphasizing stumbling blocks and contingencies of identifying and refining taxol over the ways that plant biochemists, environmentalists, corporate product managers, local collection labor, cancer advocacy groups, and others mutually shaped fundamental assumptions about the biochemistry of natural products, as well as the final entity—Taxol®.

Nevertheless, *The Story of Taxol* will be of great interest to researchers on biodiversity and screening programs, historians of cancer chemotherapy and the pharmaceutical industry, and those interested in the development of the CRADA Corporation and similar state-corporate partnership initiatives.

LINDA F. HOGLE

James R. Chiles. *Inviting Disaster: Lessons from the Edge of Technology.* xiv + 338 pp., illus., bibl., index. New York: HarperCollins Publishers, 2001. \$28 (cloth); \$15.95 (paper).

James Chiles's *Inviting Disaster* takes as its topic the vulnerability of complex technological systems to catastrophic failure in the light of human limits and human error. It is not a historical study as much as an extended essay that draws on historical cases for evidence.

The book is organized around categories of human failures, cognitive mistakes, and perceptual errors, with each chapter presenting an account of several invited disasters that fit the category—typically including a contemporary disaster involving a complex technology and a disaster from an earlier era with less widespread but no less drastic consequences. One of the author's important theses is that complex systems warrant sophisticated institutional and personal safeguards. This includes fostering a modern culture of responsibility and preparedness for the inevitable problems that occur in complex and powerful systems, as symbolized by the *Challenger* disaster, the release of toxic gases in Bhopal, and the nuclear accidents at Chernobyl and Three Mile Island. The author also addresses, with perhaps necessarily fewer examples and less detail, cases where calamity was avoided and seeks to draw from them a set of morals or lessons learned as a basis for "surviving and thriving on the new frontier" (p. 275).

All together, *Inviting Disaster* chronicles some sixty-two disasters, calamities, and near misses in a vivid and robust style that unfortunately verges on docudrama. For example, in describing a natural gas explosion that claimed nearly three hundred lives at the New London School in Texas in 1937, Chiles tells us: "The school was big, at 253 feet long, and arranged in the shape of an *E. E* for 'explosive'" (p. 156). Readers of *ISIS* will more likely want to put this book on their recreational reading list than their professional one.

Nonetheless, the book is interesting to the historian of science in several respects. Although it is unfootnoted, each chapter does provide a list of key sources, which are primarily journalistic. There is also a useful list and synopsis of the sixty-two cases cited. Thus *Inviting Disaster* has some usefulness as a compendium and sourcebook for anyone wishing a quick survey of a potential research site. More interestingly, the book employs a systems approach to its popular history of technology that would be familiar in a more scholarly context to the noted historian

of technology Thomas Hughes. Indeed, Chiles's explanatory concept of "system fracture" (p. 7 ff.) would seem to be a robust analytical concept on the order of Hughes's "seamless web" and "reverse salient." Chiles's recommendations for change appear to be based on an underlying understanding of technology as culture.

Inviting Disaster is a light read for historians, but a generally thoughtful and even entertaining one. The argument is sometimes repetitious, and the author's technique of shifting scenes from a contemporary to a more remote disaster is somewhat distracting—it reminds one of the plot technique used in the *Love Boat* television series. But then, historians were not Chiles's intended audience. The book was intended for a general audience, particularly those who may be actors in a technoscience network and who may benefit from Chiles's "lessons from the edge of technology."

J. SCOTT HAUGER

Gary L. Harris. *The Origins and Technology of the Advanced Extravehicular Space Suit.* (AAS History Series, 24.) 558 pp., illus., figs., tables, notes, index. San Diego, Calif.: Univelt, Inc., 2001. \$85 (cloth); \$60 (paper).

The History Committee of the American Astronautical Society brings us this rich, carefully researched, and well-referenced book. Gary L. Harris spent ten years tracking the evolution of the extravehicular space suit. The result is an eight-chapter book that is amenable to selective reading, although one could read it from beginning to end for an evolutionary view. *The Origins and Technology of the Advanced Extravehicular Space Suit* is an outstanding technical compendium for one's reference library. While the author describes it as "introductory in nature," many will find it several notches toward the moderately advanced side. The bibliography offers twenty-two pages of references.

The sequential reader is taken through a section on EVA (extravehicular activity) suit nomenclature, followed by a discussion of pressure-suit mechanics. Suit physiology in relation to the space environment is summarized. The EVA suit developments through the Apollo program are compared to Russian developments, including those in connection with the Mir Space Station. The advanced EVA suit requirements and technological concepts for lunar and Mars investigations are presented.

The book is richly illustrated, including more than thirty-five line drawings by the author. Many persons will find this book to be a signifi-

cant review of the EVA suit and a contribution to documenting the progress in enabling humans to work in the vacuum of space (earth orbit and lunar locations) and the thin atmosphere of Mars (with a surface atmospheric pressure less than 1 percent that of the earth).

Wiley Post, the 1934 progenitor of the first practical pressure suit and pioneer high-altitude flyer using it, would, I am sure, be very pleased to see the spectacular evolution of his early equipment to what is described in this highly readable and authoritative book.

STANLEY R. MOHLER

Brenda Denzler. *The Lure of the Edge: Scientific Passions, Religious Beliefs, and the Pursuit of UFOs.* 335 pp., notes, bibl., index. Berkeley: University of California Press, 2001. \$35 (cloth).

Brenda Denzler, who based *The Lure of the Edge* on her doctoral dissertation in religious studies at Duke University, reminds her readers that according to a 1996 Gallup poll, 48 percent of U.S. citizens believe in the reality of UFOs and 12 percent claim to "have seen a UFO" (pp. 2, 161). Despite these striking figures and the immense number of publications that the UFO movement has produced, relatively few scholars have studied the movement itself. Denzler states that the goal of her book is for the "reader to come away with a sense of the conflicting and paradoxical dimensions—and, I feel, the importance—of UFOs and the UFO movement in American society" (p. vi). Her particular focus is "the tension between science and religion as explanatory frameworks within the UFO community" (p. xv).

Put differently, her goal is to use sociological and historical techniques as well as methods employed in scholarly studies of religion in order to show that the widespread belief that the UFO movement can be conceptualized as primarily cultish in character is seriously inadequate. In pursuit of this goal, she documents in detail that the dominant orientation of many early investigators of UFO claims was scientific. Gradually, however, two developments complicated this tendency. The first was that some of the leaders in these investigations—for example, the prominent astronomer J. Allen Hynek—began to believe that the conceptual tools of science might be inadequate to uncover the compelling evidence of UFOs that he so energetically sought. Second, during the past twenty years or so, the type of evidence that presented itself shifted very significantly. In particular, hundreds if not thousands of people began to come forward claiming that they had been abducted by UFOs. Moreover,

their claims were judged credible by some respected intellectuals—for example, the Pulitzer Prize-winning Harvard psychiatrist John Mack. One investigator described this situation as nothing less than an “invisible epidemic” (p. 55), while others suggested that in the United States alone the number of UFO abductions might be as high as 3.7 million (p. 56). The upshot of these two trends was that ufology tended to split into two camps, one seeking evidence in terms of traditional scientific categories and methods, the other stressing that more capacious categories should be sought in such areas as religion, spirituality, metaphysics, and mysticism.

Denzler's book provides a well-informed and thoroughly documented history and sociology of the UFO movement in the United States. This is formulated into five main chapters: “A Short History of the UFO Myth,” “A Short History of Alien Encounters,” “Ufology: On the Cutting Edge or the Fringe of Science?” “Ufology and the Imaginal,” and “Ufology, God-Talk, and Theology.” These are supplemented by a largely empirical appendix, “A Picture of the UFO Community,” which reports on an extensive survey Denzler conducted among persons active in the movement. Her book is especially effective in sorting out the ways in which the movement has interacted with religion. She provides a sensitive and nuanced analysis of its links with theosophy, spiritualism, traditional religions, and much else. Denzler designed her volume as a work of scholarship, as is indicated by the fact that nearly half of it consists of extensive endnotes, a detailed bibliography, and a useful index. And she navigates her way through the minefield that is her subject with a commendable objectivity and a dexterity rarely found in such studies. Although it is clear that her sympathies tend toward the ufologists, she carefully presents arguments made by the “debunkers.” Historians of science interested in the history of the UFO movement or in historical interactions of science and religion should find this a useful and engaging contribution to the literature.

MICHAEL J. CROWE

■ Collections

Carl Mitcham (Editor). *Metaphysics, Epistemology, and Technology*. (Research in Philosophy and Technology, 19.) 364 pp., figs., index. Amsterdam: Elsevier Science, 2000.

Agustin A. Araya: Changed Encounters with Things and Ontological Transformations: The Case of Ubiquitous Computing. **Mary Bloodsworth**: The Implica-

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